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## PLEASANT WAYS IN SCIENCE

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# PLEASANT WAYS IN SCIENCE

BY

RICHARD A. PROCTOR

AUTHOR OF

"ROUGH WAYS MADE SMOOTH," "THE EXPANSE OF HEAVEN," "OUR PLACE  
AMONG INFINITIES," "MYTHS AND MARVELS OF ASTRONOMY"

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## P R E F A C E.

IT is very necessary that all who desire to become really proficient in any department of science should follow the beaten track, toiling more or less painfully over the difficult parts of the high road which is their only trustworthy approach to the learning they desire to attain. But there are many who wish to learn about scientific discoveries without this special labour, for which some have, perhaps, little taste, while many have scant leisure. My purpose in the present work, as in my "Light Science for Leisure Hours," the "Myths and Marvels of Astronomy," the "Borderland of Science," and "Science Byways," has been to provide paths of easy access to the knowledge of some of the more interesting discoveries, researches, or inquiries of the science of the day. I wish it to be distinctly understood that my purpose is to interest rather than to instruct, in the strict sense of the word. But I may add that it seems to me even more necessary to be cautious, and accurate in such a work as the present than in advanced treatises. For in a scientific work the reasoning which accompanies the statements of fact

affords the means of testing and sometimes of correcting such statements. In a work like the present, where explanation and description take the place of reasoning, there is no such check. For this reason I have been very careful in the accounts which I have given of the subjects here dealt with. I have been particularly careful not to present, as established truths, such views as are at present only matters of opinion.

The essays in the present volume are taken chiefly from the *Contemporary Review*, the *Gentleman's Magazine*, the *Cornhill Magazine*, *Belgravia*, and *Chambers' Journal*. The sixth, however, presents the substance (and official report) of a lecture which I delivered at the Royal Institution in May, 1870. It was then that I first publicly enunciated the views respecting the stellar universe which I afterwards more fully stated in my "Universe of Stars." The same views have been within the last twelve months submitted to the Paris Academy of Science, as the results of his own investigations, by M. Flammarion, in words which read almost like translations of passages in the above-mentioned essay.

RICHARD A. PROCTOR.

LONDON, October, 1878.

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# PLEASANT WAYS IN SCIENCE.

## *OXYGEN IN THE SUN.*

THE most promising result of solar research since Kirchhoff in 1859 interpreted the dark lines of the sun's spectrum has recently been announced from America. Interesting in itself, the discovery just made is doubly interesting in what it seems to promise in the future. Just as Kirchhoff's great discovery, that a certain double dark line in the solar spectrum is due to the vapour of sodium in the sun's atmosphere, was but the first of a long series of results which the spectroscopic analysis of the sun was to reveal, so the discovery just announced that a certain important gas—the oxygen present in our air and the chief chemical constituent of water—shows its presence in the sun by bright lines instead of dark, will in all probability turn out to be but the firstfruits of a new method of examining the solar spectrum. As its author, Dr. Henry Draper, of New York, remarks, further investigation in the direction he has pursued will lead to the discovery of other elements in the sun, but it was not "proper to conceal, for the sake of personal advantage, the principle on which such researches are to be conducted." It may well happen, though I anticipate otherwise, that by thus at once describing his method of observation, Dr. Draper may enable others to add to the

*Dark line  
vapour of  
sodium*

*Bright  
lines  
oxygen  
gas.*

list of known solar elements some which yet remain to be detected ; but if Dr. Draper should thus have added but one element to that list, he will ever be regarded as the physicist to whose acumen the method was due by which all were detected, and to whom, therefore, the chief credit of their discovery must certainly be attributed.

I propose briefly to consider the circumstances which preceded the great discovery which it is now my pleasing duty to describe, in order that the reader may the more readily follow the remarks by which I shall endeavour to indicate some of the results which seem to follow from the discovery, as well as the line along which, in my opinion, the new method may most hopefully be followed.

*Vapours  
Sodium*

It is generally known that what is called the spectroscopic method of analyzing the sun's substance had its origin in Kirchhoff's interpretation of the dark lines in the solar spectrum. Until 1859 these dark lines had not been supposed to have any special significance, or rather it had not been supposed that their significance, whatever it might be, could be interpreted. A physicist of some eminence spoke of these phenomena in 1858 in a tone which ought by the way seldom to be adopted by the man of science. "The phenomena defy, as we have seen," he said, "all attempts hitherto to reduce them within empirical laws, and no complete explanation or theory of them is possible. All that theory can be expected to do is this—it may explain how dark lines of any sort may arise within the spectrum." Kirchhoff, in 1859, showed not only how dark lines of any sort may appear, but how and why they do appear, and precisely what they mean. He found that the dark lines of the solar spectrum are due to the vapours of various elements in the sun's atmosphere, and that the nature of such elements may be determined from the observed position of the dark lines. Thus when iron is raised by the passage of the electric spark to so intense a degree of heat that it is vaporized, the light of the glowing vapour of iron is found to give a multitude of bright lines along the whole

length of the spectrum—that is, some red, some orange, some yellow, and so on. In the solar spectrum corresponding dark lines are found along the whole length of the spectrum—that is, some in the red, some in the orange, yellow, etc., and precisely in those parts of these various spectral regions which the bright lines of glowing iron would occupy. Multitudes of other dark lines exist of course in the solar spectrum. But those corresponding to the bright lines of glowing iron are unquestionably there. They are by no means lost in the multitude, as might be expected ; but, owing to the peculiarity of their arrangement, strength, etc., they are perfectly recognizable as the iron lines reversed, that is, dark instead of bright. Kirchhoff's researches showed how this is to be interpreted. It means that the vapour of iron exists in the atmosphere of the sun, glowing necessarily with an intensely bright light ; *but*, being cooler (however intensely hot) than the general mass of the sun within, the iron vapour absorbs more light than it emits, and the result is that the iron lines, instead of appearing bright, as they would if the iron vapour alone were shining, appear relatively dark on the bright rainbow-tinted background of the solar spectrum.

Thus was it shown that in the atmosphere of the sun there is the glowing vapour of the familiar metal, iron ; and in like manner other metals, and one element (hydrogen) which is not ordinarily regarded as a metal, were shown to be present in the sun's atmosphere. In saying that they are present in the sun's atmosphere, I am, in point of fact, saying that they are present in the sun ; for the solar atmosphere is, in fact, the outer part of the sun himself, since a very large part, if not by far the greater part, of the sun's mass must be vaporous. But no other elements, except the metals iron, sodium, barium, calcium, magnesium, aluminium, manganese, chromium, cobalt, nickel, zinc, copper, and titanium, and the element hydrogen, were shown to be present in the sun, by this method of observing directly the solar dark lines. In passing, I may note that there are

reasons for regarding hydrogen as a metallic element, strange though the idea may seem to those who regard hardness, brightness, malleability, ductility, plasticity, and the like, as the characteristic properties of metals, and necessarily fail to comprehend how a gas far rarer, under the same conditions, than the air we breathe, and which cannot possibly be malleable, ductile, or the like, can conceivably be regarded as a metal. But there is in reality no necessary connection between any one of the above properties and the metallic nature ; many of the fifty-five metals are wanting in all of these properties ; nor is there any reason why, as we have in mercury a metal which at ordinary temperatures is a liquid, so we might have in hydrogen a metal which, at all obtainable temperatures, and under all obtainable conditions of pressure, is gaseous. It was shown by the late Professor Graham (aided in his researches most effectively by Dr. Chandler Roberts) that hydrogen will enter into such combination with the metal palladium that it may be regarded as forming, for the time, with the palladium, an alloy ; and as alloys can only be regarded as compounds of two or more metals, the inference is that hydrogen is in reality a metallic element.

Fourteen only of the elements known to us, or less than a quarter of the total number, were thus found to be present in the sun's constitution ; and of these all were metals, if we regard hydrogen as metallic. Neither gold nor silver shows any trace of its presence, nor can any sign be seen of platinum, lead, and mercury. But, most remarkable of all, and most perplexing, was the absence of all trace of oxygen and nitrogen, two gases which could not be supposed wanting in the substance of the great ruling centre of the planetary system. It might well be believed, indeed, that none of the five metals just named are absent from the sun, and indeed that every one of the forty metals not recognized by the spectroscopic method nevertheless exists in the sun. For according to the nebular hypothesis of the origin of our solar system, the sun might be expected to contain all the

elements which exist in our earth. Some of these elements might indeed escape discovery, because existing only in small quantities ; and others (as platinum, gold, and lead, for example), because but a small portion of their vaporous substance rose above the level of that glowing surface which is called the photosphere. But that oxygen, which constitutes so large a portion of the solid, liquid, and vaporous mass of our earth, should not exist in enormous quantities, and its presence be very readily discernable, seemed amazing indeed. Nitrogen, also, might well be expected to be recognizable in the sun. Carbon, again, is so important a constituent of the earth, that we should expect to discover clear traces of its existence in the sun. In less degree, similar considerations apply to sulphur, boron, silicon, and the other non-metallic elements.

It was not supposed, however, by any one at all competent to form an opinion on the subject, that oxygen, nitrogen, and carbon are absent from the sun. It was perceived that an element might exist in enormous quantities in the substance of the sun, and yet fail to give any evidence of its presence, or only give such evidence as might readily escape recognition. If we remember how the dark lines are really caused, we shall perceive that this is so. A glowing vapour in the atmosphere of the sun absorbs rays of the same colour as it emits. If, then, it is cooler than the glowing mass of the sun which it enwraps, and if, notwithstanding the heat received from this mass, it remains cooler, then it suffers none of those rays to pass earthwards.\* It emits rays of the same kind (that is, of the same *colour*) itself, but, being cooler, the rays thus coming from it are feebler ; or, to speak more

\* More strictly, it plays the same part as a glass sereen before a glowing fire. When the heat of the fire falls on such a sereen (through whieh light passes readily enough), it is reeeived by the glass, warming the glass up to a certain point, and the warmed glass emits in all direc-tions the heat so reeeived ; thus scattering over a large space the rays whieh, but for the glass, would have fallen directly upon the objects whieh the screen is intended to protect.

correctly, the ethereal waves thus originated are feebler than those of the same order which *would* have travelled earthwards from the sun but for the interposed screen of vapour. Hence the corresponding parts of the solar spectrum are less brilliant, and contrasted with the rainbow-tinted streak of light, on which they lie as on a background, they appear dark.

In order, then, that any element may be detected by its dark lines, it is necessary that it should lie as a vaporous screen between the more intensely heated mass of the sun and the eye of the observer on earth. It must then form an enclosing envelope cooler than the sun within it. Or rather, some part of the vapour must be thus situated. For enormous masses of the vapour might be within the photospheric surface of the sun at a much higher temperature, which yet, being enclosed in the cooler vaporous shell of the same substance, would not be able to send its light rays earthwards. One may compare the state of things, so far as that particular element is concerned, to what is presented in the case of a metallic globe cooled on the outside but intensely hot within. The cool outside of such a globe is what determines the light and heat received from it, so long as the more heated mass within has not yet (by conduction) warmed the exterior shell. So in the case of a vapour permeating the entire mass, perhaps, of the sun, and at as high a temperature as the sun everywhere except on the outside: it is the temperature of the outermost part of such a vaporous mass which determines the intensity of the rays received from it—or in other words, determines whether the corresponding parts of the spectrum shall be darker or not than the rest of the spectrum. If the vapour does not rise above the photosphere of the sun in sufficient quantity to exercise a recognizable absorptive effect, its presence in the sun will not be indicated by any dark lines.

I dwell here on the question of quantity, which is sometimes overlooked in considering the spectroscopic evidence of the sun's condition, but is in reality a very important factor in determining the nature of the evidence relating to

each element in the solar mass. In some cases, the quantity of a material necessary to give unmistakable spectroscopic evidence is singularly small; insomuch that new elements, as thallium, cæsium, rubidium, and gallium, have been actually first recognized by their spectral lines when existing in such minute quantities in the substances examined as to give no other trace whatever of their existence. But it would be altogether a mistake to suppose that some element existing in exceedingly small quantities, or, more correctly, existing in the form of an exceedingly rare vapour in the sun's atmosphere, would be detected by means of its dark lines, or *by any other method depending on the study of the solar spectrum.* When we place a small portion of some substance in the space between the carbon points of an electric lamp, and volatilize that substance in the voltaic arc, we obtain a spectrum including all the bright lines of the various elements contained in the substance; and if some element is contained in it in exceedingly small quantity, we may yet perceive its distinctive bright lines among the others (many of them far brighter) belonging to the elements present in greater quantities. But if we have (for example) a great mass of molten iron, the rainbow-tinted spectrum of whose light we examine from a great distance, and if a small quantity of sodium, or other substance which vaporizes at moderate temperatures, be cast into the molten iron so that the vapour of the added element presently rises above the glowing surface of the iron, no trace of the presence of this vapour would be shown in the spectrum observed from a distance. The part of the spectrum where the dark lines of sodium usually appear would, undoubtedly, be less brilliant than before, in the same sense that the sun may be said to be less brilliant when the air is in the least degree moist than when it is perfectly dry; but the loss of brilliancy is as utterly imperceptible in the one case as it is in the other. In like manner, a vapour might exist in the atmosphere of the sun (above the photosphere, that is), of whose presence not a trace would be afforded in the spectroscope, for the simple reason that the absorptive

action of the vapour, though exerted to reduce the brightness of particular solar rays or tints, would not affect those rays sufficiently for the spectroscopist to recognize any diminution of their lustre.

There is another consideration, which, so far as I know, has not hitherto received much attention, but should certainly be taken into account in the attempt to interpret the real meaning of the solar spectrum. Some of the metals which are vaporized by the sun's heat below the photosphere may become liquid or even solid at or near the level of the photosphere. Even though the heat at the level of the photosphere may be such that, under ordinary conditions of pressure and so forth, such metals would be vaporous, the enormous pressure which must exist not far below the level of the photosphere may make the heat necessary for complete vaporization far greater than the actual heat at that level. In that case the vapour will in part condense into liquid globules, or, if the heat is considerably less than is necessary to keep the substance in the form of vapour, then it may in part be solidified, the tiny globules of liquid metal becoming tiny crystals of solid metal. We see both conditions fulfilled within the limits of our own air in the case of the vapour of water. Low down the water is present in the air (ordinarily) in the form of pure vapour ; at a higher level the vapour is condensed by cold into liquid drops forming visible clouds (cumulus clouds), and yet higher, where the cold is still greater, the minute water-drops turn into ice-crystals, forming those light fleecy clouds called cirrus clouds by the meteorologist. Now true clouds of either sort may exist in the solar atmosphere even above that photospheric level which forms the boundary of the sun we see. It may be said that the spectroscope, applied to examine matter outside the photosphere, has given evidence only of vaporous cloud masses. The ruddy prominences which tower tens of thousands of miles above the surface of the sun, and the sierra (or as it is sometimes unclassically called, the chromosphere) which covers usually the whole of the photosphere to a

depth of about eight thousand miles, show only, under spectroscopic scrutiny, the bright lines indicating gaseity. But though this is perfectly true, it is also true that we have not here a particle of evidence to show that clouds of liquid particles, and of tiny crystals, may not float over the sun's surface, or even that the ruddy clouds shown by the spectroscope to shine with light indicative of gaseity may not also contain liquid and crystalline particles. For in point of fact, the very principle on which our recognition of the bright lines depends involves the inference that matter whose light would *not* be resolved into bright lines would not be recognizable at all. The bright lines are seen, because by means of a spectroscope we can throw them far apart, without reducing their lustre, while the background of rainbow-tinted spectrum has its various portions similarly thrown further apart and correspondingly weakened. One may compare the process (the comparison, I believe, has not hitherto been employed) to the dilution of a dense liquid in which solid masses have been floating : the more we increase the quantity of the liquid in diluting it with water, the more transparent it becomes, but the solid masses in it are not changed, so that we only have to dilute the liquid sufficiently to see these masses. *But* if there were in the interstices of the solid masses particles of some substance which dissolved in the water, we should not recognize the presence of this substance by any increase in its visibility ; for the very same process which thinned the liquid would thin this soluble substance in the same degree. In like manner, by dispersing and correspondingly weakening the sun's light more and more, we can recognize the light of the gaseous matter in the prominences, for this is not weakened ; but if the prominences also contain matter in the solid or liquid form (that is, drops or crystals), the spectroscopic method will not indicate the presence of such matter, for the spectrum of matter of this sort will be weakened by dispersion in precisely the same degree that the solar spectrum itself is weakened.

It is easy to see how the evidence of the presence of any

element which behaved in this way would be weakened, if we consider what would happen in the case of our own earth, according as the air were simply moist but without clouds, or loaded with cumulus masses but without cirrus clouds, or loaded with cirrus clouds. For although there is not in the case of the earth a central glowing mass like the sun's, on whose rainbow-tinted spectrum the dark lines caused by the absorptive action of our atmosphere could be seen by the inhabitant of some distant planet studying the earth from without, yet the sun's light reflected from the surface of the earth plays in reality a similar part. It does not give a simple rainbow-tinted spectrum ; for, being sunlight, it shows all the dark lines of the solar spectrum : but the addition of new dark lines to these, in consequence of the absorptive action of the earth's atmosphere, could very readily be determined. In fact, we do thus recognize in the spectra of Mars, Venus, and other planets, the presence of aqueous vapour in their atmosphere, despite the fact that our own air, containing also aqueous vapour, naturally renders so much the more difficult the detection of that vapour in the atmosphere of remote planets necessarily seen through our own air. Now, a distant observer examining the light of our own earth on a day when, though the air was moist, there were no clouds, would have ample evidence of the presence of the vapour of water ; for the light which he examined would have gone twice through our earth's atmosphere, from its outermost thinnest parts to the densest layers close to the surface, then back again through the entire thickness of the air. But if the air were heavily laden with cumulus clouds (without any cirrus clouds at a higher layer), although *we* should know that there was abundant moisture in the air, and indeed much more moisture then there had been when there had been no clouds, our imagined observer would either perceive no traces at all of this moisture, or he would perceive traces so much fainter than when the air was clear that he would be apt to infer that the air was either quite dry, or at least very much drier than

it had been in that case. For the light which he would receive from the earth would not in this case have passed through the entire depth of moisture-laden air twice, but twice only through that portion of the air which lay above the clouds, at whose surface the sun's light would be reflected. The whole of the moisture-laden layer of the air would be snugly concealed under the cloud-layer, and would exercise no absorptive action whatever on the light which the remote observer would examine. If from the upper surface of the layer of cumulus clouds aqueous vapour rose still higher, and were converted in the cold upper regions of the atmosphere into clouds of ice-crystals, the distant observer would have still less chance of recognizing the presence of moisture in our atmosphere. For the layer of air between the cumulus clouds and the cirrus clouds would be unable to exert any absorptive action on the light which reached the observer. All such light would come to him after reflection from the layer of cirrus clouds. He would be apt to infer that there was no moisture at all in the air of our planet, at the very time when in fact there was so much moisture that not one layer only, but two layers of clouds enveloped the earth, the innermost layer consisting of particles of liquid water, the outermost of particles of frozen water. Using the words ice, water, and steam, to represent the solid, liquid, and vaporous states of water, we may fairly say that ice and water, by hiding steam, would persuade the remote observer that there was no water at all on the earth—at least if he trusted solely to the spectroscopic evidence then obtained.\*

\* The case here imagined is not entirely hypothetical. We examine Mercury and Venus very nearly under the conditions here imagined ; for we can obtain only spectroscopic evidence respecting the existence of water on either planet. In the case of Mars we have telescopic evidence, and no one now doubts that the greenish parts of the planet are seas and oceans. But Venus and Mercury are never seen under conditions enabling the observer to determine the colour of various parts of their discs.

I may add that a mistake, somewhat analogous to that which I have described in the cases of an imagined observer of our earth, has been

We might in like manner fail to obtain any spectroscopic evidence of the presence of particular elements in the sun, because they do not exist in sufficient quantity in the vaporous form in those outer layers which the spectroscope can alone deal with.

In passing, I must note a circumstance in which some of those who have dealt with this special part of the spectroscopic evidence have erred. It is true in one sense that some elements may be of such a nature that their vapours cannot rise so high in the solar atmosphere as those of other elements. But it must not be supposed that the denser vapours seek a lower level, the lighter vapours rising higher. According to the known laws of gaseous diffusion, a gas or vapour diffuses itself throughout a space occupied by another gas or several other gases, in the same way as though the space were not occupied at all. If we introduce into a vessel full of common air a quantity of carbonic acid gas (I follow the older and more familiar nomenclature), this gas, although of much higher specific gravity than either oxygen or nitrogen, does not take its place at the bottom of the vessel, but so diffuses itself that the air of the upper part of the vessel contains exactly the same quantity of carbonic acid gas as the air of the lower

made by some spectroscopists in the case of the planets Jupiter and Saturn. In considering the spectroscopic evidence respecting the condition of these planets' atmospheres, they have overlooked the circumstance that we can judge only of the condition of the outermost and coolest layers, for the lower layers are concealed from view by the enormous cloud masses, floating, as the telescope shows, in the atmospheric envelopes of the giant planets. Thus the German spectroscopist Vögel argues that because in the spectrum of Jupiter dark lines are seen which are known to belong to the absorption-spectrum of aqueous vapour, the planet's surface cannot be intensely hot. But Jupiter's absorption-spectrum belongs to layers of his atmosphere lying far above his surface. We can no more infer the actual temperature of Jupiter's surface from the temperature of the layers which produce his absorption-spectrum, than a visitor who should view our earth from outer space, observing the low temperature of the air ten or twelve miles above the sea-level, could infer thence the actual temperature of the earth's surface.

part. Similarly, if hydrogen is introduced, it does not seek the upper part of the vessel, but diffuses itself uniformly throughout the vessel. If we enclose the carbonic acid gas in a light silken covering, and the hydrogen in another (at the same pressure as the air in the vessel) one little balloon will sink and the other will rise; but this is simply because diffusion is prevented. It may be asked how this agrees with what I have said above, that some elements may not exist in sufficient quantity or in suitable condition above the sun's photospheric level to give any spectroscope evidence of their nature. As to quantity, indeed, the answer is obvious: if there is only a small quantity of any given element in the entire mass of the sun, only a very small quantity can under any circumstances exist outside the photosphere. As regards condition, it must be remembered that the vessel of my illustrative case was supposed to contain air at a given temperature and pressure throughout. If the vessel was so large that in different parts of it the temperature and pressure were different, the diffusion would, indeed, still be perfect, because at all ordinary temperatures and pressures hydrogen and carbonic acid gas remain gaseous. But if the vapour introduced is of such a nature that at moderate temperatures and pressures it condenses, wholly or in part, or liquefies, the diffusion will not take place with the same uniformity. We need not go further for illustration than to the case of our own atmosphere as it actually exists. The vapour of water spreads uniformly through each layer of the atmosphere which is at such a temperature and pressure as to permit of such diffusion; but where the temperature is too low for complete diffusion (at the actual pressure) the aqueous vapour is condensed into visible cloud, diffusion being checked at this point as at an impassable boundary. In the case of the sun, as in the case of our own earth, it is not the density of an element when in a vaporous form which limits its diffusion, but the value of the temperature at which its vapour at given pressure condenses into liquid particles. It is in this way only that any separation can be

effected between the various elements which exist in the sun's substance. A separation of this sort is unquestionably competent to modify the spectroscopic evidence respecting different elements. But it would be a mistake to suppose that any such separation could occur as has been imagined by some—a separation causing in remote times the planets supposed to have been thrown off by the sun to be rarest on the outskirts of the solar system and densest close to the sun. The small densities of the outer family of planets, as compared with the densities of the so-called terrestrial planets, must certainly be otherwise explained.

But undoubtedly the chief circumstance likely to operate in veiling the existence of important constituents of the solar mass must be that which has so long prevented spectroscopists from detecting the presence of oxygen in the sun. An element may exist in such a condition, either over particular parts of the photosphere, or over the entire surface of the sun, that instead of causing dark lines in the solar spectrum it may produce bright lines. Such lines may be conspicuous, or they may be so little brighter than the background of the spectrum as to be scarcely perceptible or quite imperceptible.

In passing, I would notice that this interpretation of the want of all spectroscopic evidence of the presence of oxygen, carbon, and other elements in the sun, is not an *ex post facto* explanation. As will presently appear, it is now absolutely certain that oxygen, though really existing, and doubtless, in enormous quantities, in the sun, has been concealed from recognition in this way. But that this might be so was perceived long ago. I myself, in the first edition of my treatise on "The Sun," pointed out, in 1870, with special reference to nitrogen and oxygen, that an element "may be in a condition enabling it to radiate as much light as it absorbs, or else very little more or very little less; so that it either obliterates all signs of its existence, or else gives lines so little brighter or darker than the surrounding parts of the spectrum that we can detect no trace of its

existence." I had still earlier given a similar explanation of the absence of all spectroscopic evidence of hydrogen in the case of the bright star Betelgeux.\*

Let us more closely consider the significance of what we learn from the spectral evidence respecting the gas hydrogen. We know that when the total light of the sun is dealt with, the presence of hydrogen is constantly indicated by dark lines. In other words, regarding the sun as a whole, hydrogen constantly reduces the emission of rays of those special tints which correspond to the light of this element. When we examine the light of other suns than ours, we find that in many cases, probably in by far the greater number of cases, hydrogen acts a similar part. But not in every case. In the spectra of some stars, notably in those of Betelgeux and Alpha Herculis, the lines of hydrogen are not visible at all; while in yet others, as Gamma Cassiopeiae, the middle star of the five which form the straggling W of this constellation, the lines of hydrogen show bright upon the relatively dark background of the spectrum. When we examine closely the sun himself, we find that although his light as a whole gives a spectrum in which the lines of hydrogen appear dark, the light of particular parts of his surface, if separately examined, occasionally shows the hydrogen lines bright as in the spectrum of Gamma Cassiopeiae, while sometimes the light of particular parts gives,

\* In "Other Worlds than Ours," I wrote as follows:—"The lines of hydrogen, which are so well marked in the solar spectrum, are not seen in the spectrum of Betelgeux. We are not to conclude from this that hydrogen does not exist in the composition of the star. We know that certain parts of the solar disc, when examined with the spectroscope, do not at all times exhibit the hydrogen lines, or may even present them as bright instead of dark lines. It may well be that in Betelgeux hydrogen exists under such conditions that the amount of light it sends forth is nearly equivalent to the amount it absorbs, in which case its characteristic lines would not be easily discernible. In fact, it is important to notice generally, that while there can be no mistaking the positive evidence afforded by the spectroscope as to the existence of any element in sun or star, the negative evidence supplied by the absence of particular lines is not to be certainly relied upon."

like the light of Betelgeux, no spectroscopic evidence whatever of the presence of hydrogen. Manifestly, if the whole surface of the sun were in the condition of the portions which give bright hydrogen lines, the spectrum of the sun would resemble that of Gamma Cassiopeiae; while if the whole surface were in the condition of those parts which show no lines of hydrogen, the spectrum of the sun would resemble that of Betelgeux. Now if there were any reason for supposing that the parts of the sun which give no lines of hydrogen are those from which the hydrogen has been temporarily removed in some way, we might reasonably infer that in the stars whose spectra show no hydrogen lines there is no hydrogen. But the fact that the hydrogen lines are sometimes seen bright renders this supposition untenable. For we cannot suppose that the lines of hydrogen change from dark to bright or from bright to dark (both which changes certainly take place) without passing through a stage in which they are neither bright nor dark; in other words, we are compelled to assume that there is an intermediate condition in which the hydrogen lines, though really existent, are invisible because they are of precisely the same lustre as the adjacent parts of the spectrum. Hence the evanescence of the hydrogen lines affords no reason for supposing that hydrogen has become even reduced in quantity where the lines are not seen. And therefore it follows that the invisibility of the hydrogen lines in the spectrum of Betelgeux is no proof that hydrogen does not exist in that star in quantities resembling those in which it is present in the sun. And this, being demonstrated in the case of one gas, must be regarded as at least probable in the case of other gases. Wherefore the absence of the lines of oxygen from the spectrum of any star affords no sufficient reason for believing that oxygen is not present in that star, or that it may not be as plentifully present as hydrogen, or even far more plentifully present.

There are other considerations which have to be taken into account, as well in dealing with the difficulty arising

from the absence of the lines of particular elements from the solar spectrum as in weighing the extremely important discovery which has just been effected by Dr. H. Draper.

I would specially call attention now to a point which I thus presented seven years ago :—“The great difficulty of interpreting the results of the spectroscopic analysis of the sun arises from the circumstance that we have no means of learning whence that part of the light comes which gives the continuous spectrum. When we recognize certain dark lines, we know certainly that the corresponding element exists in the gaseous form at a lower temperature than the substance which gives the continuous spectrum. But as regards that continuous spectrum itself we can form no such exact opinion.” It might, for instance, have its origin in glowing liquid or solid matter; but it might also be compounded of many spectra, each containing a large number of bands, the bands of one spectrum filling up the spaces which would be left dark between the bands of another spectrum, and so on until the entire range from the extreme visible red to the extreme visible violet were occupied by what appeared as a continuous rainbow-tinted streak. “We have, in fact, in the sun,” as I pointed out, “a vast agglomeration of elements, subject to two giant influences, producing in some sort opposing effects—viz., a temperature far surpassing any we can form any conception of, and a pressure (throughout nearly the whole of the sun’s globe) which is perhaps even more disproportionate to the phenomena of our experience. Each known element would be vaporized by the solar temperature at known pressures; each (there can be little question) would be solidified by the vast pressures, did these arise at known temperatures. Now whether, under these circumstances, the laws of gaseous diffusion prevail where the elements *are* gaseous in the solar globe; whether, where liquid matter exists it is in general bounded in a definite manner from the neighbouring gaseous matter; whether any elements at all are solid, and if so under what conditions their solidity is maintained and the

limits of the solid matter defined—all these are questions which *must* be answered before we can form a satisfactory idea of the solar constitution ; yet they are questions which we have at present no means of answering." Again, we require to know whether any process resembling combustion can under any circumstances take place in the sun's globe. If we could assume that some general resemblance exists between the processes at work upon the sun and those we are acquainted with, we might imagine that the various elements ordinarily exist in the sun's globe in the gaseous form (chiefly) to certain levels, to others chiefly in the liquid form, and to yet others chiefly in the solid form. But even then that part of each element which is gaseous may exist in two forms, having widely different spectra (in reality in five, but I consider only the extreme forms). The light of one part is capable of giving characteristic spectra of lines or bands (which will be different according to pressure and may appear either dark or bright) ; that of the other is capable of giving a spectrum nearly or quite continuous.

It will be seen that Dr. H. Draper's discovery supplies an answer to one of the questions, or rather to one of the sets of questions, thus indicated. I give his discovery as far as possible in his own words.

"*Oxygen discloses itself,*" he says, "*by bright lines or bands in the solar spectrum,* and does not give dark absorption-lines like the metals. We must therefore change our theory of the solar spectrum, and no longer regard it merely as a continuous spectrum with certain rays absorbed by a layer of ignited metallic vapours, but as having also bright lines and bands superposed on the background of continuous spectrum. Such a conception not only opens the way to the discovery of others of the non-metals, sulphur, phosphorus, selenium, chlorine, bromine, iodine, fluorine, carbon, etc., but also may account for some of the so-called dark lines, by regarding them as intervals between bright lines. It must be distinctly understood that in speaking of the solar spectrum here, I do not mean the spectrum of any

limited area upon the disc or margin of the sun, but the spectrum of light from the whole disc."

In support of the important statement here advanced, Dr. Draper submits a photograph of part of the solar spectrum with a comparison spectrum of air, and also with some of the lines of iron and aluminium. The photograph itself, a copy of which, kindly sent to me by Dr. Draper, lies before me as I write, fully bears out Dr. Draper's statement. It is absolutely free from handwork or retouching, except that reference letters have been added in the negative. It shows the part of the solar spectrum between the well-known Fraunhofer lines G and H, of which G (an iron line) lies in the indigo, and H (a line of hydrogen) in the violet, so that the portion photographed belongs to that region of the spectrum whose chemical or actinic energy is strongest. Adjacent to this lies the photograph of the air lines, showing nine or ten well-defined oxygen lines or groups of lines, and two nitrogen bands. The exact agreement of the two spectra in position is indicated by the coincidence of bright lines of iron and aluminium included in the air spectrum with the dark lines of the same elements in the solar spectrum. "No close observation," as Dr. Draper truly remarks, "is needed to demonstrate to even the most casual observer" (of this photograph) "that the oxygen lines are found in the sun as bright lines." There is in particular one quadruple group of oxygen lines in the air spectrum, the coincidence of which with a group of bright lines in the solar spectrum is unmistakable.

"This oxygen group alone is almost sufficient," says Dr. Draper, "to prove the presence of oxygen in the sun, for not only does each of the four components have a representative in the solar group, but the relative strength and the general aspect of the lines in each case is similar.\* I shall not

\* Dr. Draper remarks here in passing, "I do not think that, in comparisons of the spectra of the elements and sun, enough stress has been laid on the general appearance of lines apart from their mere position; in photographic representations this point is very prominent."

attempt at this time," he proceeds, "to give a complete list of the oxygen lines, . . . and it will be noticed that some lines in the air spectrum which have bright analogues in the sun are not marked with the symbol of oxygen. This is because there has not yet been an opportunity to make the necessary detailed comparisons. In order to be certain that a line belongs to oxygen, I have compared, under various pressures, the spectra of air, oxygen, nitrogen, carbonic acid, carburetted hydrogen, hydrogen, and cyanogen.

"As to the spectrum of nitrogen and the existence of this element in the sun there is not yet certainty. Nevertheless, even by comparing the diffused nitrogen lines of this particular photograph, in which nitrogen has been sacrificed to get the best effect for oxygen, the character of the evidence appears. There is a triple band somewhat diffused in the photograph belonging to nitrogen, which has its appropriate representative in the solar spectrum, and another band of nitrogen is similarly represented." Dr. Draper states that "in another photograph a heavy nitrogen line which in the present one lies opposite an insufficiently exposed part of the solar spectrum, corresponds to a comparison band in the sun."

But one of the most remarkable points in Dr. Draper's paper is what he tells us respecting the visibility of these lines in the spectrum itself. They fall, as I have mentioned, in a part of the spectrum where the actinic energy is great but the luminosity small; in fact, while this part of the spectrum is the very strongest for photography, it is close to the region of the visible spectrum,

"Where the last gleamings of refracted light  
Die in the fainting violet away."

It is therefore to be expected that those, if any, of the bright lines of oxygen, will be least favourably shown for direct vision, and most favourably for what might almost be called photographic vision, where we see what photography records for us. Yet Dr. Draper states that these bright lines of

oxygen can be readily seen. "The bright lines of oxygen in the spectrum of the solar disc have not been hitherto perceived, probably from the fact that in eye-observation bright lines on a less bright background do not make the impression on the mind that dark lines do. When attention is called to their presence they are readily enough seen, even without the aid of a reference spectrum. The photograph, however, brings them into greater prominence." As the lines of oxygen are not confined to the indigo and violet, we may fairly hope that the bright lines in other parts of the spectrum of oxygen may be detected in the spectrum of the sun, now that spectroscopists know that bright lines and not dark lines are to be looked for.

Dr. Draper remarks that from purely theoretic considerations derived from terrestrial chemistry, and the nebular hypothesis, the presence of oxygen in the sun might have been strongly suspected; for this element is currently stated to form eight-ninths of the water of the globe, one-third of the crust of the earth, and one-fifth of the air, and should therefore probably be a large constituent of every member of the solar system. On the other hand, the discovery of oxygen, and probably other non-metals, in the sun gives increased strength to the nebular hypothesis, because to many persons the absence of this important group has presented a considerable difficulty. I have already remarked on the circumstance that we cannot, according to the known laws of gaseous diffusion, accept the reasoning of those who have endeavoured to explain the small density of the outer planets by the supposition that the lighter gases were left behind by the great contracting nebulous mass, out of which, on the nebular hypothesis, the solar system is supposed to have been formed. It is important to notice, now, that if on the one hand we find in the community of structure between the sun and our earth, as confirmed by the discovery of oxygen and nitrogen in the sun, evidence favouring the theory according to which all the members of that system were formed out of what was originally a single mass, we do

not find evidence against the theory (as those who have advanced the explanation above referred to may be disposed to imagine) in the recognition in the sun's mass of enormous quantities of one of these elements which, according to their view, ought to be found chiefly in the outer members of the solar system. If those who believe in the nebular hypothesis (generally, that is, for many of the details of the hypothesis as advanced by Laplace are entirely untenable in the present position of physical science) had accepted the attempted explanation of the supposed absence of the non-metallic elements in the sun, they would now find themselves in a somewhat awkward position. They would, in fact, be almost bound logically to reject the nebular hypothesis, seeing that one of the asserted results of the formation of our system, according to that hypothesis, would have been disproved. But so far as I know no supporter of the nebular hypothesis possessing sufficient knowledge of astronomical facts and physical laws to render his opinion of any weight, has ever given in his adhesion to the unsatisfactory explanation referred to.

The view which I have long entertained respecting the growth of the solar system—viz., that it had its origin, not in contraction only or chiefly, but in combined processes of contraction and accretion—seems to me to be very strongly confirmed by Dr. Draper's discovery. This would not be the place for a full discussion of the reasons on which this opinion is based. But I may remark that I believe no one who applies the laws of physics, *as at present known*, to the theory of the simple contraction of a great nebulous mass formerly extending far beyond the orbit of Neptune, till, when planet after planet had been thrown off, the sun was left in his present form and condition in the centre, will fail to perceive enormous difficulties in the hypothesis, or to recognize in Dr. Draper's discovery a difficulty added to those affecting the hypothesis *so presented*. Has it ever occurred, I often wonder, to those who glibly quote the nebular theory as originally propounded, to inquire how far some of the pro-

cesses suggested by Laplace are in accordance with the now known laws of physics? To begin with, the original nebulous mass extending to a distance exceeding the earth's distance from the sun more than thirty times (this being only the distance of Neptune), if we assign to it a degree of compression making its axial diameter half its equatorial diameter, would have had a volume exceeding the sun's (roughly) about 120,000,000,000 times, and in this degree its mean density would have been less than the sun's. This would correspond to a density equal (roughly) to about one-400,000th part of the density of hydrogen gas at atmospheric pressure. To suppose that a great mass of matter, having this exceedingly small mean density, and extending to a distance of three or four thousand millions of miles from its centre, could under any circumstances rotate as a whole, or behave in other respects after the fashion attributed to the gaseous embryo of the solar system in ordinary descriptions of the nebular hypothesis, is altogether inconsistent with correct ideas of physical and dynamical laws. It is absolutely a necessity of any nebular hypothesis of the solar system, that from the very beginning a central nucleus and subordinate nuclei should form in it, and grow according to the results of the motions (at first to all intents and purposes independent) of its various parts. Granting this state of things, we arrive, by considering the combined effects of accretion and contraction, at a process of development according fully as well as that ordinarily described with the general relations described by Laplace, and accounting also (in a general way) for certain peculiarities which are in no degree explained by the ordinary theory. Amongst these may specially be noted the arrangement and distribution of the masses within the solar system, and the fact that so far as spectroscopic evidence enables us to judge, a general similarity of structure exists throughout the whole of the system.

Inquiring as to the significance of his discovery, Dr. Draper remarks that it seems rather difficult "at first sight

to believe that an ignited\* gas in the solar atmosphere should not be indicated by dark lines in the solar spectrum, and should appear not to act under the law, 'a gas when ignited absorbs rays of the same refrangibility as those it emits.' But, in fact, the substances hitherto investigated in the sun are really metallic vapours, hydrogen probably coming under that rule. The non-metals obviously may behave differently. It is easy to speculate on the causes of such behaviour ; and it may be suggested that the reason of the non-appearance of a dark line may be that the intensity of the light from a great thickness of ignited oxygen overpowers the effect of the photosphere, just as, if a person were to look at a candle-flame through a yard thickness of sodium vapour, he would only see bright sodium lines, and no dark absorption."

The reasoning here is not altogether satisfactory (or else is not quite correctly expressed). In the first place, the difficulty dealt with has no real existence. The law that a gas when glowing absorbs rays of the same refrangibility as it emits, does not imply that a gas between a source of light and the observer will show its presence by spectroscopic dark lines. A gas so placed *does* receive from the source of light rays corresponding to those which it emits itself, if it is cooler than the source of light ; and it absorbs them, being in fact heated by means of them, though the gain of temperature may be dissipated as fast as received ; but if the gas is hotter, it emits more of those rays than it absorbs, and will make its presence known by its bright lines. This is not a matter of speculation, but of experiment. On

\* The word "ignited" may mislead, and indeed is not correctly used here. The oxygen in the solar atmosphere, like the hydrogen, is simply glowing with intensity of heat. No process of combustion is taking place. Ignition, strictly speaking, means the initiation of the process of combustion, and a substance can only be said to be ignited when it has been set burning. The word *glowing* is preferable ; or if reference is made to heat and light combined, then "glowing with intensity of heat" seems the description most likely to be correctly understood.

the other hand, the experiment suggested by Dr. Draper would not have the effect he supposes, if it were correctly made. Doubtless, if the light from a considerable area of dully glowing sodium vapour were received by the spectroscope at the same time as the light of a candle-flame seen through the sodium vapour, the light of the sodium vapour overcoming that of the candle-flame would indicate its presence by bright lines; but if light were received only from that portion of the sodium vapour which lay between the eye and the candle-flame, then I apprehend that the dark lines of sodium would not only be seen, but would be conspicuous by their darkness.

It is in no cavilling spirit that I indicate what appears to me erroneous in a portion of Dr. Draper's reasoning on his great discovery. The entire significance of the discovery depends on the meaning attached to it, and therefore it is most desirable to ascertain what this meaning really is. There can be no doubt, I think, that we are to look for the true interpretation of the brightness of the oxygen lines in the higher temperature of the oxygen, not in the great depth of oxygen above the photospheric level. The oxygen which produces these bright lines need not necessarily be above the photosphere at all. (In fact, I may remark here that Dr. Draper, in a communication addressed to myself, mentions that he has found no traces at present of oxygen above the photosphere, though I had not this circumstance in my thoughts in reasoning down to the conclusion that the part of the oxygen effective in showing these bright lines lies probably below the visible photosphere.) Of course, if the photosphere were really composed of glowing solid and liquid matter, or of masses of gas so compressed and so intensely heated as to give a continuous spectrum, no gas existing below the photosphere could send its light through, nor could its presence, therefore, be indicated in any spectroscopic manner. But the investigations which have been made into the structure of the photosphere as revealed by the telescope, and in particular the observations made by

Professor Langley, of the Alleghany Observatory, show that we have not in the photosphere a definite bounding envelope of the sun, but receive light from many different depths below that spherical surface, 425,000 miles from the sun's centre, which we call the photospheric level. We receive more light from the centre of the solar disc, I feel satisfied, not solely because the absorptive layer through which we there see the sun is shallower, but partly, and perhaps chiefly, because we there receive light from some of the interior and more intensely heated parts of the sun.\* Should this prove to be the case, it may be found possible to do what heretofore astronomers have supposed to be impossible—to ascertain in some degree how far and in what way the constitution of the sun varies below the photosphere, which, so far as ordinary telescopic observation is concerned, seems to present a limit below which researches cannot be pursued.

I hope we shall soon obtain news from Dr. Huggins's Observatory that the oxygen lines have been photographed, and possibly the bright lines of other elements recognized in the solar spectrum. Mr. Lockyer also, we may hope, will exercise that observing skill which enabled him early to recognize the presence of bright hydrogen lines in the spectrum of portions of the sun's surface, to examine that spectrum for other bright lines.

I do not remember any time within the last twenty years when the prospects of fresh solar discoveries seemed more hopeful than they do at present. The interest which has of late years been drawn to the subject has had the effect of

\* It would be an interesting experiment, which I would specially recommend to those who, like Dr. Draper, possess instrumental means specially adapted to the inquiry, to ascertain what variations, if any, occur in the solar spectrum when (i.) the central part of the disc alone, and (ii.) the outer part alone, is allowed to transmit light to the spectroscope. The inquiry seems specially suited to the methods of spectral photography pursued by Dr. Draper, and by Dr. Huggins, in this country. Still, I believe interesting results can be obtained even without these special appliances; and I hope before long to employ my own telescope in this department of research.

enlisting fresh recruits in the work of observation, and many of these may before long be heard of as among those who have employed Dr. Draper's method successfully.

But I would specially call attention to the interest which attaches to Dr. Draper's discovery and to the researches likely to follow from it, in connection with a branch of research which is becoming more and more closely connected year by year with solar investigations—I mean stellar spectroscopy. We have seen the stars divided into orders according to their constitution. We recognize evidence tending to show that these various orders depend in part upon age—not absolute but relative age. There are among the suns which people space some younger by far than our sun, others far older, and some in a late stage of stellar decrepitude. Whether as yet spectroscopists have perfectly succeeded in classifying these stellar orders in such sort that the connection between a star's spectrum and the star's age can be at once determined, may be doubtful. But certainly there are reasons for hoping that before long this will be done. Amongst the stars, and (strange to say) among celestial objects which are not stars; there are suns in every conceivable stage of development, from embryon masses not as yet justly to be regarded as suns, to masses which have ceased to fulfil the duties of suns. Among the more pressing duties of spectroscopic analysis at the present time is the proper classification of these various orders of stars. Whensoever that task shall have been accomplished, strong light, I venture to predict, will be thrown on our sun's present condition, as well as on his past history, and on that future fate upon which depends the future of our earth.

## *SUN-SPOT, STORM, AND FAMINE.*

DURING the last five or six years a section of the scientific world has been exercised with the question how far the condition of the sun's surface with regard to spots affects our earth's condition as to weather, and therefore as to those circumstances which are more or less dependent on weather. Unfortunately, the question thus raised has not presented itself alone, but in company with another not so strictly scientific, in fact, regarded by most men of science as closely related to personal considerations—the question, namely, whether certain indicated persons should or should not be commissioned to undertake the inquiry into the scientific problem. But the scientific question itself ought not to be less interesting to us because it has been associated, correctly or not, with the wants and wishes of those who advocate the endowment of science. I propose here to consider the subject in its scientific aspect only, and apart from any bias suggested by the appeals which have been addressed to the administrators of the public funds.

It is hardly necessary to point out, in the first place, that all the phenomena of weather are directly referable to the sun as their governing cause. His rays poured upon our air cause the more important atmospheric currents directly. Indirectly they cause modifications of these currents, because where they fall on water or on moist surfaces they raise aqueous vapour into the air, which, when it returns to the liquid form as cloud, gives up to the surrounding air the heat which had originally vaporized the water. In these

ways, directly or indirectly, various degrees of pressure and temperature are brought about in the atmospheric envelope of the earth, and, speaking generally, all air currents, from the gentlest zephyr to the fiercest tornado, are the movements by which the equilibrium of the air is restored. Like other movements tending to restore equilibrium, the atmospheric motions are oscillatory. Precisely as when a spring has been bent one way, it flies not back only, but beyond the mean position, till it is almost equally bent the other way, so the current of air which rushes in towards a place of unduly diminished pressure does more than restore the mean pressure, so that presently a return current carries off the excess of air thus carried in. We may say, indeed, that the mean pressure at any place scarcely ever exists, and when it exists for a time the resulting calm is of short duration. Just as the usual condition of the sea surface is one of disturbance, greater or less, so the usual condition of the air at every spot on the earth's surface is one of motion not of quiescence. Every movement of the air, thus almost constantly perturbed, is due directly or indirectly to the sun.

So also every drop of rain or snow, every particle of liquid or of frozen water in mist or in cloud, owes its birth to the sun. The questions addressed of old to Job, "Hath the rain a father? or who hath begotten the drops of dew? out of whose womb came the ice? and the hoary frost of heaven, who hath gendered it?" have been answered by modern science, and to every question the answer is, The Sun. He is parent of the snow and hail, as he is of the moist warm rains of summer, of the ice which crowns the everlasting hills, and of the mist which rises from the valleys beneath his morning rays.

Since, then, the snow that clothes the earth in winter as with a garment, and the clouds that in due season drop fatness on the earth, are alike gendered by the sun; since every movement in our air, from the health-bringing breeze to the most destructive hurricane, owns him as its parent; we must at the outset admit, that if there is any body

external to the earth whose varying aspect or condition can inform us beforehand of changes which the weather is to undergo, the sun is that body. That for countless ages the moon should have been regarded as the great weather-breeder, shows only how prone men are to recognize in apparent changes the true cause of real changes, and how slight the evidence is on which they will base laws of association which have no real foundation in fact. Every one can see when the moon is full, or horned, or gibbous, or half-full; when her horns are directed upwards, or downwards, or sideways. And as the weather is always changing, even as the moon is always changing, it must needs happen that from time to time changes of weather so closely follow changes of the moon as to suggest that the two orders of change stand to each other in the relation of cause and effect. Thus rough rules (such as those which Aratus has handed down to us) came to be formed, and as (to use Bacon's expression) men mark when such rules hit, and never mark when they miss, a system of weather lore gradually comes into being, which, while in one sense based on facts, has not in reality a particle of true evidence in its favour—every single fact noted for each relation having been contradicted by several unnoted facts opposed to the relation. There could be no more instructive illustration of men's habits in such matters than the system of lunar weather wisdom in vogue to this day among seamen, though long since utterly disproved by science. But let it be remarked in passing, that in leaving the moon, which has no direct influence, and scarcely any indirect influence, on the weather, for the sun, which is all-powerful, we have not got rid of the mental habits which led men so far astray in former times. We shall have to be specially careful lest it lead us astray yet once more, perhaps all the more readily because of the confidence with which we feel that, at the outset anyway, we are on the right road.

I suppose there must have been a time when men were not altogether certain whether the varying apparent path of

the sun, as he travels from east to west every day, has any special effect on the weather. It seems so natural to us to recognize in the sun's greater mid-day elevation and longer continuance above the horizon in summer, the cause of the greater warmth which then commonly prevails, that we find it difficult to believe that men could ever have been in doubt on this subject. Yet it is probable that a long time passed after the position of the sun as ruler of the day had been noticed, before his power as ruler of the seasons was recognized. I cannot at this moment recall any passage in the Bible, for example, in which direct reference is made to the sun's special influence in bringing about the seasons, or any passage in very ancient writings referring definitely to the fact that the weather changes with the changing position of the sun in the skies (as distinguished from the star-sphere), and with the changing length of the day. "While the earth remaineth," we are told in Genesis, "seed-time and harvest, and cold and heat, and summer and winter, and day and night, shall not cease;" but there is no reference to the sun's aspect as determining summer and winter. We find no mention of any of the celestial signs of the seasons anywhere in the Bible, I think, but such signs as are mentioned in the parable of the fig tree—"When his branch is yet tender, and putteth forth leaves, ye know that summer is nigh." Whether this indicates or not that the terrestrial, rather than the celestial signs of the progress of the year were chiefly noted by men in those times, it is tolerably certain that in the beginning a long interval must have elapsed between the recognition of the seasons themselves, and the recognition of their origin in the changes of the sun's apparent motions.

When this discovery was effected, men made the most important and, I think, the most satisfactory step towards the determination of cyclic associations between solar and terrestrial phenomena. It is for that reason that I refer specially to the point. In reality, it does not appertain to my subject, for seasons and sun-spots are not associated. But it admir-

ably illustrates the value of cyclic relations. Men might have gone on for centuries, we may conceive, noting the recurrence of seed-time and harvest-time, summer and winter, recognizing the periodical returns of heat and cold, and (in some regions) of dry seasons and wet seasons, of calm and storm, and so forth, without perceiving that the sun runs through his changes of diurnal motion in the same cyclic period. We can imagine that some few who might notice the connection between the two orders of celestial phenomena would be anxious to spread their faith in the association among their less observant brethren. They might maintain that observatories for watching the motions of the sun would demonstrate either that their belief was just or that it was not so, would in fact dispose finally of the question. It is giving the most advantageous possible position to those who now advocate the erection of solar observatories for determining what connection, if any, may exist between sun-spots and terrestrial phenomena, thus to compare them to observers who had noted a relation which unquestionably exists. But it is worthy of notice that if those whom I have imagined thus urging the erection of an observatory for solving the question whether the sun rules the seasons, and to some degree regulates the recurrence of dry or rainy, and of calm or stormy weather, had promised results of material value from their observations, they would have promised more than they could possibly have performed. Even in this most favourable case, where the sun is, beyond all question, the efficient ruling body, where the nature of the cyclic change is most exactly determinable, and where even the way in which the sun acts can be exactly ascertained, no direct benefit accrues from the knowledge. The exact determination of the sun's apparent motions has its value, and this value is great, but it is most certainly not derived from any power of predicting the recurrence of those phenomena which nevertheless depend directly on the sun's action. The farmer who in any given year knows from the almanac the exact duration of

daylight, and the exact mid-day elevation of the sun for every day in the year, is not one whit better able to protect his crops or his herds against storm or flood than the tiller of the soil or the tender of flocks a hundred thousand years or so ago, who knew only when seed-time and summer and harvest-time and winter were at hand or in progress.

The evidence thus afforded is by no means promising, then, so far as the prediction of special storms, or floods, or droughts is concerned. It would seem that if past experience can afford any evidence in such matters, men may expect to recognize cycles of weather change long before they recognize corresponding solar cycles (presuming always that such cycles exist), and that they may expect to find the recognition of such association utterly barren, so far as the possibility of predicting definite weather changes is concerned. It would seem that there is no likelihood of anything better than what Sir J. Herschel said *might* be hoped for hereafter. "A lucky hit may be made; nay, some rude approach to the perception of a 'cycle of seasons' may *possibly* be obtainable. But no person in his senses would alter his plans of conduct for six months in advance in the most trifling particular on the faith of any special prediction of a warm or a cold, a wet or a dry, a calm or a stormy, summer or winter"—far less of a great storm or flood announced for any special day.

But let us see what the cycle association between solar spots and terrestrial weather actually is, or rather of what nature it promises to be, for as yet the true nature of the association has not been made out.

It has been found that in a period of about eleven years the sun's surface is affected by what may be described as a wave of sun-spots. There is a short time—a year or so—during which scarce any spots are seen; they become more and more numerous during the next four or five years, until they attain a maximum of frequency and size; after this they wane in number and dimensions, until at length, about eleven years from the time when he had before been freest from

spots, he attains again a similar condition. After this the spots begin to return, gradually attain to a maximum, then gradually diminish, until after eleven more years have elapsed few or none are seen. It must not be supposed that the sun is always free from spots at the time of minimum spot frequency, or that he always shows many and large spots at the time of maximum spot frequency. Occasionally several very large spots, and sometimes singularly large spots, have been seen in the very heart of the minimum spot season, and again there have been occasions when scarcely any spots have been seen for several days in the very heart of the maximum spot season. But, taking the average of each year, the progression of the spots in number and frequency from minimum to maximum, and their decline from maximum to minimum, are quite unmistakeable.

Now there are some terrestrial phenomena which we might expect to respond in greater or less degree to the sun's changes of condition with respect to spots. We cannot doubt that the emission both of light and of heat is affected by the presence of spots. It is not altogether clear in what way the emission is affected. We cannot at once assume that because the spots are dark the quantity of sun-light must be less when the spots are numerous; for it may well be that the rest of the sun's surface may at such times be notably brighter than usual, and the total emission of light may be greater on the whole instead of less. Similarly of the emission of heat. It is certain that when there are many spots the surface of the sun is far less uniform in brightness than at other times. The increase of brightness all round the spots is obvious to the eye when the sun's image, duly enlarged, is received upon a screen in a darkened room. Whether the total emission of light is increased or diminished has not yet been put to the test. Professor Langley, of the Alleghany Observatory, near Pittsburg, U.S., has carefully measured the diminution of the sun's emission of light and heat on the assumption that the portion of the surface not marked by spots remains unchanged in lustre. But until

the total emission of light and heat at the times of maximum and minimum has been measured, without any assumption of the kind, we cannot decide the question.

More satisfactory would seem to be the measurements which have been made by Professor Piazzi Smyth, at Edinburgh, and later by the Astronomer Royal at Greenwich, into the underground temperature of the earth. By examining the temperature deep down below the surface, all local and temporary causes of change are eliminated, and causes external to the earth can alone be regarded as effective in producing systematic changes. "The effect is very slight," I wrote a few years ago, "indeed barely recognizable. I have before me as I write Professor Smyth's sheet of the quarterly temperatures from 1837 to 1869 at depths of 3, 6, 12, and 24 French feet. Of course the most remarkable feature, even at the depth of 24 feet, is the alternate rise and fall with the seasons. But it is seen that, while the range of rise and fall remains very nearly constant, the crests and troughs of the waves lie at varying levels." After describing in the essay above referred to, which appears in my "Science Byways," the actual configuration of the curves of temperature both for seasons and for years, and the chart in which the sun-spot waves and the temperature waves are brought into comparison, I was obliged to admit that the alleged association between the sun-spot period and the changes of underground temperature did not seem to me very clearly made out. It appears, however, there is a slight increase of temperature at the time when the sun-spots are least numerous.

That the earth's magnetism is affected by the sun's condition with respect to spots, seems to have been more clearly made out, though it must be noted that the Astronomer Royal considers the Greenwich magnetic observations inconsistent with this theory. It seems to have been rendered at least extremely probable that the daily oscillation of the magnetic needle is greater when spots are numerous than when there are few spots or none. Magnetic

storms are also more numerous at the time of maximum spot-frequency, and auroras are then more common. (The reader will not fall into the mistake of supposing that magnetic storms have the remotest resemblance to hurricanes, or rainstorms, or hailstorms, or even to thunderstorms, though the thunderstorm is an electrical phenomenon. What is meant by a magnetic storm is simply such a condition of the earth's frame that the magnetic currents traversing it are unusually strong.)

Thus far, however, we have merely considered relations which we might fairly expect to find affected by the sun's condition as to spots. A slight change in his total brightness and in the total amount of heat emitted by him may naturally be looked for under circumstances which visibly affect the emission of light, and presumably affect the emission of heat also, from portions of his surface. Nor can we wonder if terrestrial magnetism, which is directly dependent on the sun's emission of heat, should be affected by the existence of spots upon his surface.

It is otherwise with the effects which have recently been associated with the sun's condition. It may or may not prove actually to be the case that wind and rain vary in quantity as the sun-spots vary in number (at least when we take in both cases the average for a year, or for two or three years), but it cannot be said that any such relation was antecedently to be expected. When we consider what the sun actually does for our earth, it seems unlikely that special effects such as these should depend on relatively minute peculiarities of the sun's surface. There is our earth, with her oceans and continents, turning around swiftly on her axis, and exposed to his rays as a whole. Or, inverting the way of viewing matters, there is the sun riding high in the heavens of any region of the earth, pouring down his rays upon that region. We can understand how in the one case that rotating orb of the earth may receive rather more or rather less heat from the sun when he is spotted than when he is not, or how in the other way of viewing matters, that

orb of the sun may give to any region rather more or rather less heat according as his surface is more or less spotted. But that in special regions of that rotating earth storms should be more or less frequent or rainfall heavier or lighter, as the sun's condition changes through the exceedingly small range of variation due to the formation of spots, seems antecedently altogether unlikely ; and equally unlikely the idea that peculiarities affecting limited regions of the sun's surface should affect appreciably the general condition of the earth. If a somewhat homely comparison may be permitted, we can well understand how a piece of meat roasting before a fire may receive a greater or less supply of heat on the whole as the fire undergoes slight local changes (very slight indeed they must be, that the illustration may be accurate) ; but it would be extremely surprising if, in consequence of such slight changes in the fire, the roasting of particular portions of the joint were markedly accelerated or delayed, or affected in any other special manner.

But of course all such considerations as to antecedent probabilities must give way before the actual evidence of observed facts. Utterly inconsistent with all that is yet known of the sun's physical action, as it may seem, on *a priori* grounds, to suppose that spots, currents, or other local disturbances of the sun's surface could produce any but general effects on the earth as a whole, yet if we shall find that particular effects are produced in special regions of the earth's surface in cycles unmistakably synchronizing with the solar-spot-cycle, we must accept the fact, whether we can explain it or not. Only let it be remembered at the outset that the earth is a large place, and the variations of wind and calm, rain and drought, are many and various in different regions. Whatever place we select for examining the rainfall, for example, we are likely to find, in running over the records of the last thirty years or so, some seemingly oscillatory changes ; in the records of the winds, again, we are likely to find other seemingly oscillatory changes ; if none of these records provide anything which seems in any way to corres-

pond with the solar spot-cycle, we may perchance find some such cycle in the relative rainfall of particular months, or in the varying wetness or dryness of particular winds, and so forth. Or, if we utterly fail to find any such relation in one place we may find it in another, or not improbably in half-a-dozen places among the hundreds which are available for the search. If we are content with imperfect correspondence between some meteorological process or another and the solar-spot cycle, we shall be exceedingly unfortunate indeed if we fail to find a score of illustrative instances. And if we only record these, without noticing any of the cases where we fail to find any association whatever—in other words, as Bacon puts it, if “we note when we hit and never note when we miss,” we shall be able to make what will seem a very strong case indeed. But this is not exactly the scientific method in such cases. By following such a course, indeed, we might prove almost anything. If we take, for instance, a pack of cards, and regard the cards in order as corresponding to the years 1825 to 1877, and note their colours as dealt *once*, we shall find it very difficult to show that there is any connection whatever between the colours of the cards corresponding to particular years and the number of spots on the sun’s face. But if we repeat the process a thousand times, we shall find certain instances among the number, in which red suits correspond to all the years when there are many spots on the sun, and black suits to all the years when there are few spots on the sun. If now we were to publish all such deals, without mentioning anything at all about the others which showed no such association, we should go far to convince a certain section of the public that the condition of the sun as to spots might hereafter be foretold by the cards; whence, if the public were already satisfied that the condition of the sun specially affects the weather of particular places, it would follow that the future weather of these places might also be foretold by the cards.

I mention this matter at the outset, because many who are anxious to find some such cycle of seasons as Sir John

Herschel thought might be discovered, have somewhat overlooked the fact that we must not hunt down such a cycle *per fas et nefas*. "Surely in meteorology as in astronomy," Mr. Lockyer writes, for instance, "the thing to hunt down is a cycle, and if that is not to be found in the temperate zone, then go to the frigid zones or the torrid zone to look for it; and if found, then above all things and in whatever manner, lay hold of, study, and read it, and see what it means." There can be no doubt that this is the way to find a cycle, or at least to find what looks like a cycle, but the worth of a cycle found in this way will be very questionable.\*

I would not have it understood, however, that I consider all the cycles now to be referred to as unreal, or even that the supposed connection between them and the solar cycle has no existence. I only note that there are thousands, if not tens of thousands, of relations among which cycles may be looked for, and that there are perhaps twenty or thirty cases in which some sort of cyclic association between certain meteorological relations and the period of the solar spots presents itself. According to the recognized laws of probability, some at least amongst these cases must be regarded as accidental. Some, however, may still remain which are not accidental.

Among the earliest published instances may be mentioned Mr. Baxendell's recognition of the fact that during a certain series of years, about thirty, I think, the amount of rainfall at Oxford was greater under west and south-west winds than under south and south-east winds when sun-spots were most numerous, whereas the reverse held in years when there were no spots or few. Examining the meteorological records of

\* In 1860, a year of maximum sun-spot frequency, Cambridge won the University boat-race; the year 1865, of minimum sun-spot frequency, marked the middle of a long array of Oxford victories; 1872, the next maximum, marked the middle of a Cambridge series of victories. May we not anticipate that in 1878, the year of minimum spot frequency, Oxford will win? [This prediction made in autumn, 1877, was fulfilled.] I doubt not similar evidence might be obtained about cricket.

St. Petersburg, he found that a contrary state of things prevailed there.

The Rev. Mr. Main, Director of the Radcliffe Observatory at Oxford, found that westerly winds were slightly more common (as compared with other winds) when sun-spots were numerous than when they were few.

Mr. Meldrum, of Mauritius, has made a series of statistical inquiries into the records of cyclones which have traversed the Indian Ocean between the equator and 34 degrees south latitude, in each year from 1856 to 1877, noting the total distances traversed by each, the sums of their radii and areas, their duration in days, the sums of their total areas, and their relative areas. His researches, be it marked in passing, are of extreme interest and value, whether the suggested connection between sun-spots and cyclones (in the region specified) be eventually found to be a real one or not. The following are his results, as described in *Nature* by a writer who manifestly favours very strongly the doctrine that an intimate association exists between solar maculation (or spottiness) and terrestrial meteorological phenomena :—

“ The period embraces two complete, or all but complete, sun-spot periods, the former beginning with 1856 and ending in 1867, and the latter extending from 1867 to about the present time [1877]. The broad result is that the number of cyclones, the length and duration of their courses, and the extent of the earth’s surface covered by them all, reach the maximum in each sun-spot period during the years of maximum maculation, and fall to the minimum during the years of minimum maculation. The peculiar value of these results lies in the fact that the portion of the earth’s surface over which this investigation extends, is, from its geographical position and what may be termed its meteorological homogeneity, singularly well fitted to bring out prominently any connection that may exist between the condition of the sun’s surface and atmospheric phenomena.”

The writer proceeds to describe an instance in which

Mr. Meldrum predicted future meteorological phenomena, though without specifying the exact extent to which Mr. Meldrum's anticipations were fulfilled or the reverse. "A drought commenced in Mauritius early in November," he says, "and Mr. Meldrum ventured (on December 21) to express publicly his opinion that probably the drought would not break up till towards the end of January, and that it might last till the middle of February, adding that up to these dates the rainfall of the island would probably not exceed 50 per cent. of the mean fall. This opinion was an inference grounded on past observations, which show that former droughts have lasted from about three to three and a half months, and that these droughts have occurred in the years of minimum sun-spots, or, at all events, in years when the spots were far below the average, such as 1842, 1843, 1855, 1856, 1864, 1866, and 1867, and that now we are near the minimum epoch of sun-spots. It was further stated that the probability of rains being brought earlier by a cyclone was but slight, seeing that the season for cyclones is not till February or March, and that no cyclone whatever visited Mauritius during 1853-56 and 1864-67, the years of minimum sun-spots. From the immense practical importance of this application of the connection between sun-spots and weather to the prediction of the character of the weather of the ensuing season, we shall look forward with the liveliest interest to a detailed statement of the weather which actually occurred in that part of the Indian Ocean from November to March last [1876]."

It was natural that the great Indian famine, occurring at a time when sun-spots were nearly at a minimum, should by some be directly associated with a deficiency of sun-spots. In this country, indeed, we have had little reason, during the last two or three years of few sun-spots, to consider that drought is one of the special consequences to be attributed to deficient solar maculation. But in India it may be different, or at least it may be different in Madras, for it has been satisfactorily proved that in some parts of India the

rainfall increases in inverse, not in direct proportion, to the extent of solar maculation. Dr. Hunter has shown to the satisfaction of many that at Madras there is "a cycle of rainfall corresponding with the period of solar maculation." But Mr. E. D. Archibald, who is also thoroughly satisfied that the sun-spots affect the weather, remarks that Dr. Hunter has been somewhat hasty in arguing that the same conditions apply throughout the whole of Southern India. "This hasty generalization from the results of one station situated in a vast continent, the rainfall of which varies completely, both in amount and the season in which it falls, according to locality, has been strongly contested by Mr. Blanford, the Government Meteorologist, who, in making a careful comparison of the rainfalls of seven stations, three of which (Madras, Bangalore, and Mysore) are in Southern India, the others being Bombay, Najpore, Jubbulpore, and Calcutta, finds that, with the exception of Najpore in Central India, which shows some slight approach to the same cyclical variation which is so distinctly marked in the Madras registers, the rest of the stations form complete exceptions to the rule adduced for Madras, in many of them the hypothetical order of relation being reversed. Mr. Blanford, however, shows that, underlying the above irregularities, a certain cyclical variation exists on the average at all the stations, the amount, nevertheless, being so insignificant (not more than 9 per cent. of the total falls) that it could not be considered of sufficient magnitude to become a direct factor in the production of famine. It thus appears that the cycle of rainfall which is considered to be the most important element in causing periodic famines has only been proved satisfactorily for the town of Madras. It may perhaps hold for the Carnatic and Northern Siccars, the country immediately surrounding Madras, though perhaps, owing to the want of rainfall registers in these districts, evidence with regard to this part is still wanting." On this Mr. Archibald proceeds to remark that, though Dr. Hunter has been only partially successful, the value of his

able pamphlet is not diminished in any way, "an indirect effect of which has been to stimulate meteorological inquiry and research in the same direction throughout India. The meteorology of this country (India), from its peculiar and tropical position, is in such complete unison with any changes that may arise from oscillations in the amount of solar radiation, and their effects upon the velocity and direction of the vapour-bearing winds, that a careful study of it cannot fail to discover meteorological periodicities in close connection with corresponding periods of solar disturbance." So, indeed, it would seem.

The hope that famines may be abated, or, at least, some of their most grievous consequences forestalled by means of solar observatories, does not appear very clearly made out. Rather it would seem that the proper thing to do is to investigate the meteorological records of different Indian regions, and consider the resulting evidence of cyclic changes without any special reference to sun-spots; for if sun-spots may cause drought in one place, heavy rainfall in another, winds here and calms there, it seems conceivable that the effects of sun-spots may differ at different times, as they manifestly do in different places.

Let us turn, however, from famines to shipwrecks. Perhaps, if we admit that cyclones are more numerous, and blow more fiercely, and range more widely, even though it be over one large oceanic region only, in the sun-spot seasons than at other times, we may be assured, without further research, that shipwrecks will, on the whole, be more numerous near the time of sun-spot maxima than near the time of sun-spot minima.

The idea that this may be so was vaguely shadowed forth in a poem of many stanzas, called "The Meteorology of the Future: a Vision," which appeared in *Nature* for July 5, 1877. I do not profess to understand precisely what the object of this poem may have been—I mean, whether it is intended to support or not the theory that sun-spots influence the weather. Several stanzas are very

humorous, but the object of the humour is not manifest. The part referred to above is as follows:—Poor Jack lies at the bottom of the sea in 1881, and is asked in a spiritual way various questions as to the cause of his thus coming to grief. This he attributed to the rottenness of the ship in which he sailed, to the jobbery of the inspector, to the failure of the system of weather telegraphing, and so forth. But, says the questioner, there was one

“In fame to none will yield,  
He led the band who reaped renown  
On India’s famine field.

“Was he the man to see thee die?  
Thou wilt not tax him—come?  
The dead man groaned—‘*I met my death*  
*Through a sun-spot maximum.*’”

The first definite enunciation, however, of a relation between sun-spots and shipwrecks appeared in September, 1876. Mr. Henry Jeula, in the *Times* for September 19, stated that Dr. Hunter’s researches into the Madras rainfall had led him to throw together the scanty materials available relating to losses posted on Lloyd’s loss book, to ascertain if any coincidences existed between the varying number of such losses and Dr. Hunter’s results. “For,” he proceeds, “since the cycle of rainfall at Madras coincides, I am informed, with the periodicity of the cyclones in the adjoining Bay of Bengal” (a relation which is more than doubtful) “as worked out by the Government Astronomer at Mauritius” (whose researches, however, as we have seen, related to a region remote from the Bay of Bengal), “some coincidence between maritime casualties, rainfalls, and sun-spots appeared at least possible.” In passing, I may note that if any such relation were established, it would be only an extension of the significance of the cycle of cyclones, and could have no independent value. It would certainly follow, if the cycle of cyclones is made out, that shipwrecks being more numerous, merchants would suffer, and we should

have the influence of the solar spots asserting itself in the *Gazette*. From the cyclic derangement of monetary and mercantile matters, again, other relations also cyclic in character would arise. But as all these may be inferred from the cycle of cyclones once this is established, we could scarcely find in their occurrence fresh evidence of the necessity of that much begged-for solar observatory. The last great monetary panic in this country, by the way, occurred in 1866, at a time of minimum solar maculation. Have we here a decisive proof that the sun rules the money market, the bank rate of discount rising to a maximum as the sun-spots sink to a minimum, and *vice versa*? The idea is strengthened by the fact that the American panic in 1873 occurred when spots were very numerous, and its effects have steadily subsided as the spots have diminished in number; for this shows that the sun rules the money market in America on a principle diametrically opposed to that on which he (manifestly) rules the money market in England, precisely as the spots cause drought in Calcutta and plenteous rainfall at Madras, wet south-westers and dry south-westers at Oxford, and wet south-easters and dry south-easters at St. Petersburg. Surely it would be unreasonable to refuse to recognize the weight of evidence which thus tells on both sides at once.

To return, however, to the sun's influence upon shipwrecks.

Mr. Jeula was "only able to obtain data for two complete cycles of eleven years, namely, from 1855 to 1876 inclusive, while the period investigated by Dr. Hunter extended from 1813 to 1876, and his observations related to Madras and its neighbourhood only, while the losses posted at Lloyd's occurred to vessels of various countries, and happened in different parts of the world. It was necessary to bring these losses to some common basis of comparison, and the only available one was the number of 'British registered vessels of the United Kingdom and Channel Islands'—manifestly an arbitrary one. I consequently cast

out the percentage of losses posted each year upon the number of registered vessels for the same year, and also the percentage of losses posted in each of the eleven years of the two cycles upon the total posted in each complete cycle, thus obtaining two bases of comparison independent of each other."

The results may be thus presented :—

Taking the four years of each cycle when sun-spots were least in number, Mr. Jeula found the mean percentage of losses in registered vessels of the United Kingdom and Channel Islands to be 11.13, and the mean percentage of losses in the total posted in the entire cycle of eleven years to be 8.64.

In the four years when sun-spots were intermediate in number, that is in two years following the minimum and in two years preceding the minimum, the respective percentages were 11.91 and 9.21.

Lastly, in the three years when sun-spots were most numerous, these percentages were, respectively, 12.49 and 9.53.

That the reader may more clearly understand what is meant here by percentages, I explain that while the numbers 11.13, 11.91, and 12.49 simply indicate the average number of wrecks (per hundred of all the ships registered) which occurred in the several years of the eleven-years cycle, the other numbers, 8.64, 19.21, and 9.53, indicate the average number of wrecks (per hundred of wrecks recorded) during eleven successive years, which occurred in the several years of the cycle. The latter numbers seem more directly to the purpose; and as the two sets agree pretty closely, we may limit our attention to them.

Now I would in the first place point out that it would have been well if the actual number or percentage had been indicated for each year of the cycle, instead of for periods of four years, four years, and three years. Two eleven-year cycles give in any case but meagre evidence, and it would have been well if the evidence had been given as fully as

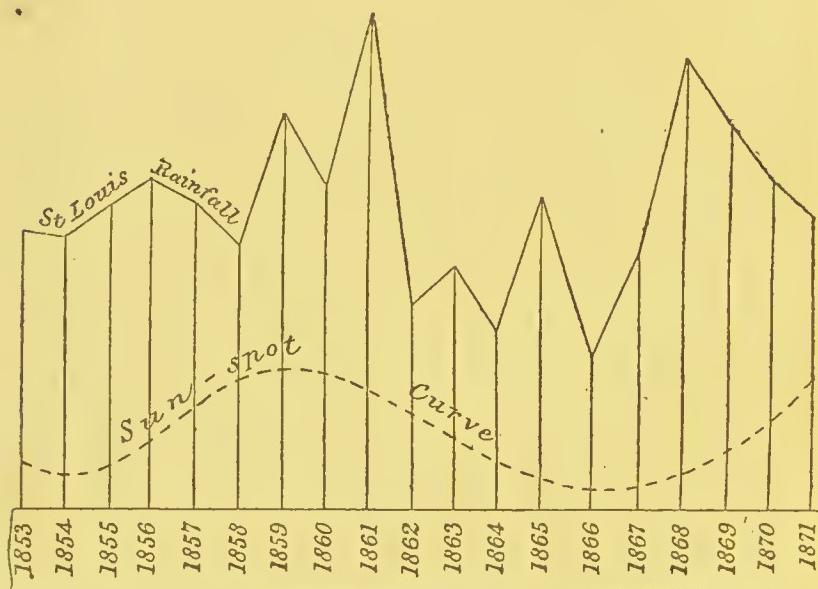
possible. If we had a hundred eleven-yearly cycles, and took the averages of wrecks for the four years of minimum solar maculation, the four intermediate years, and the three years of maximum maculation, we might rely with considerable confidence on the result, because accidental peculiarities one way or the other could be eliminated. But in two cycles only, such peculiarities may entirely mask any cyclic relation really existing, and appear to indicate a relation which has no real existence. If the percentages had been given for each year, the effect of such peculiarities would doubtless still remain, and the final result would not be more trustworthy than before; but we should have a chance of deciding whether such peculiarities really exist or not, and also of determining what their nature may be. As an instance in point, let me cite a case where, having only the results of a single cycle, we can so arrange them as to appear to indicate a cyclic association between sun-spots and rainfall, while, when we give them year by year, such an association is discredited, to say the least.

The total rainfall at Port Louis, between the years 1855 and 1868 inclusive, is as follows :—

		<i>Rainfall.</i>		<i>Condition of Sun.</i>
In 1855	...	42.665 inches	...	Sun-spot minimum.
1856	...	46.230	„	...
1857	...	43.445	„	...
1858	...	35.506	„	...
1859	...	56.875	„	...
1860	...	45.166	„	...
1861	...	68.733	„	...
1862	...	28.397	„	...
1863	...	33.420	„	...
1864	...	24.147	„	...
1865	...	44.730	„	...
1866	...	20.571	„	...
1867	...	35.970	„	...
1868	...	64.180	„	...

I think no one, looking at these numbers as they stand, can recognize any evidence of a cyclic tendency. If we represent the rainfall by ordinates we get the accompanying

figure, which shows the rainfall for eighteen years, and again I think it may be said that a very lively imagination is required to recognize anything resembling that wave-like undulation which the fundamental law of statistics requires where a cycle is to be made out from a single oscillation.



Certainly the agreement between the broken curve of rainfall and the sun-spot curve indicated by the waved dotted line is not glaringly obvious. But when we strike an average for the rainfall, in the way adopted by Mr. Jeula for shipwrecks, how pleasantly is the theory of sun-spot influence illustrated by the Port Louis rainfall ! Here is the result, as quoted by the high-priest of the new order of diviners, from the papers by Mr. Meldrum :—

Three minimum years—total rainfall	.. . . .	133.340
Three maximum years	„	170.774
Three minimum years	„	120.721

Nothing could be more satisfactory, but nothing, I venture to assert, more thoroughly inconsistent with the true method of statistical research.

May it not be that, underlying the broad results presented by Mr. Jeula, there are similar irregularities?

When we consider that the loss of ships depends, not only on a cause so irregularly variable (to all seeming) as wind-storms, but also on other matters liable to constant change, as the variations in the state of trade, the occurrence of wars and rumours of wars, special events, such as international exhibitions, and so forth, we perceive that an even wider range of survey is required to remove the effects of accidental peculiarities in their case, than in the case of rainfall, cyclones, or the like. I cannot but think, for instance, that the total number of ships lost in divers ways during the American war, and especially in its earlier years (corresponding with two of the three maximum years of sun-spots) may have been greater, not merely absolutely but relatively, than in other years. I think it conceivable, again, that during the depression following the great commercial panic of 1866 (occurring at a time of minimum solar maculation, as already noticed) the loss of ships may have been to some degree reduced, relatively as well as absolutely. We know that when trade is unusually active many ships have sailed, and perhaps may still be allowed to sail (despite Mr. Plimsoll's endeavours), which should have been broken up; whereas in times of trade depression the ships actually afloat are likely to be, *on the average*, of a better class. So also, when, for some special reason, passenger traffic at sea is abnormally increased. I merely mention these as illustrative cases of causes not (probably) dependent on sun-spots, which may (not improbably) have affected the results examined by Mr. Jeula. I think it possible that those results, if presented for each year, would have indicated the operation of such causes, naturally masked when sets of four years, four years, and three years are taken instead of single years.

I imagine that considerations such as these will have to be taken into account and disposed of before it will be unhesitatingly admitted that sun-spots have any great effect in increasing the number of shipwrecks.

The advocates of the doctrine of sun-spot influence—or, perhaps it would be more correct to say, the advocates of the endowment of sun-spot research—think differently on these and other points. Each one of the somewhat doubtful relations discussed above is constantly referred to by them as a demonstrated fact, and a demonstrative proof of the theory they advocate. For instance, Mr. Lockyer, in referring to Meldrum's statistical researches into the frequency of cyclones, does not hesitate to assert that according to these researches “the whole question of cyclones is merely a question of solar activity, and that if we wrote down in one column the number of cyclones in any given year, and in another column the number of sun-spots in any given year, there will be a strict relation between them—many sun-spots, many hurricanes ; few sun-spots, few hurricanes.” . . . And again, “Mr. Meldrum has since found” (not merely “has since found reason to believe,” but definitely, “has since found”) “that what is true of the storms which devastate the Indian Ocean is true of the storms which devastate the West Indies ; and on referring to the storms of the Indian Ocean, Mr. Meldrum points out that at those years where we have been quietly mapping the sun-spot maxima, the harbours were filled with wrecks, and vessels coming in disabled from every part of the Indian Ocean.” Again, Mr. Balfour Stewart accepts Mr. Jeula's statistics confidently as demonstrating that there are most shipwrecks during periods of maximum solar activity. Nor are the advocates of the new method of prediction at all doubtful as to the value of these relations in affording the basis of a system of prediction. They do not tell us precisely *how* we are to profit by the fact, if fact it is, that cyclones and shipwrecks mark the time of maximum solar maculation, and droughts and famine the time of minimum. “If we can manage to get at these things,” says Mr. Lockyer, “the power of prediction, that power which would be the most useful one in meteorology, if we could only get at it, would be within our grasp.” And Mr. Balfour Stewart, in a letter to the *Times*, says, “If we are on

the track of a discovery which will in time enable us to foretell the cycle of droughts, public opinion should demand that the investigation be prosecuted with redoubled vigour and under better conditions. If forewarned be forearmed, then such research will ultimately conduce to the saving of life both at times of maximum and minimum sun-spot frequency."

If these hopes are really justified by the facts of the case, it would be well that the matter should be as quickly as possible put to the test. No one would be so heartless, I think, as to reject, through an excess of scientific caution, a scheme which might issue in the saving of many lives from famine or from shipwreck. And on the other hand, no one, I think, would believe so ill of his fellow-men as to suppose for one moment that advantage could be taken of the sympathies which have been aroused by the Indian famine, or which may from time to time be excited by the record of great disasters by sea and land, to advocate bottomless schemes merely for purposes of personal advancement. We must now, perforce, believe that those who advocate the erection of new observatories and laboratories for studying the physics of the sun, have the most thorough faith in the scheme which they proffer to save our Indian population from famine and our seamen from shipwreck.

But they, on the other hand, should now also believe that those who have described the scheme as entirely hopeless, do really so regard it. If we exonerate them from the charge of responding to an appeal for food by offering spectrometers, they in turn should exonerate us from the charge of denying spectrometers to the starving millions of India though knowing well that the spectroscopic track leads straight to safety.

I must acknowledge I cannot for my own part see even that small modicum of hope in the course suggested which would suffice to justify its being followed. In my opinion, one ounce of rice would be worth more (simply because it would be worth something) than ten thousand tons of spec-

troscopes. For what, in the first place, has been shown as to the connection between meteorological phenomena and sun-spots? Supposing we grant, and it is granting a great deal, that all the cycles referred to have been made out. They one and all affect averages only. The most marked among them can so little be trusted in detail that while the maximum of sun-spots agrees *in the main* with an excess or defect of rain or wind, or of special rains with special winds, or the like, the actual year of maximum may present the exact reverse.

Of what use can it be to know, for instance, that the three years of least solar maculation will probably give a rainfall less than that for the preceding or following three years, if the middle year of the three, when the spots are most numerous of all, *may* haply show plenteous rainfall? Or it may be the first of the three, or the last, which is thus well supplied, while a defect in the other two, or in one of the others, brings the total triennial rainfall below the average. What provision could possibly be made under such circumstances to meet a contingency which may occur in any one of three years? or, at least, what provision could be made which would prove nearly so effective as an arrangement which could readily be made for keeping sufficient Government stores at suitable stations (that is, never allowing such stores to fall at the critical season in each year below a certain minimum), and sending early telegraphic information of unfavourable weather? Does any one suppose that the solar rice-grains are better worth watching for such a purpose than the terrestrial rice-grains, or that it is not well within the resources of modern science and modern means of communication and transport, to make sufficient preparation each year for a calamity always possible in India? And be it noticed that if, on the one hand, believers in solar safety from famine may urge that, in thus objecting to their scheme, I am opposing what might, in some year of great famine and small sun-spots, save the lives of a greater number than would be saved by any system of terrestrial watchfulness, I would point out, on the other, that the solar scheme, if it means anything at all,

means special watchfulness at the minimum sun-spot season, and general confidence (so far as famine is concerned) at the season of maximum solar maculation ; and that while as yet nothing has been really proved about the connection between sun-spots and famine, such confidence might prove to be a very dangerous mistake.

Supposing even it were not only proved that sun-spots exert such and such effects, but that this knowledge can avail to help us to measures of special precaution, how is the study of the sun going to advance our knowledge ? In passing, let it be remarked that already an enormous number of workers are engaged in studying the sun in every part of the world. The sun is watched on every fine day, in every quarter of the earth, with the telescope, analyzed with the spectroscope, his prominences counted and measured, his surface photographed, and so forth. What more ought to be or could be done ? But that is not the main point. If more could be done, what could be added to our knowledge which would avail in the way of prediction ? "At present," says Mr. Balfour Stewart, "the problem has not been pursued on a sufficiently large scale or in a sufficient number of places. If the attack is to be continued, the skirmishers should give way to heavy guns, and these should be brought to bear without delay now that the point of attack is known." In other words, now that we know, according to the advocates of these views, that meteorological phenomena follow roughly the great solar-spot period, we should prosecute the attack in this direction, in order to find out——what ? Minor periods, perhaps, with which meteorological phenomena may still more roughly synchronize. Other such periods are already known with which meteorological phenomena have never yet been associated. New details of the sun's surface ? No one has yet pretended that any of the details already known, except the spots, affect terrestrial weather, and the idea that peculiarities so minute as hitherto to have escaped detection can do so, is as absurd, on the face of it, as the supposition that minute details in the structure of a burning

coal, such details as could only be detected by close scrutiny, can affect the general quality and effects of the heat transmitted by the coal, as part of a large fire, to the further side of a large room.

Lastly, I would urge this general argument against a theory which seems to me to have even less to recommend it to acceptance than the faith in astrology.\* *If it requires,*

\* It must be understood that this remark relates only to the theory that by close scrutiny of the sun a power of predicting weather peculiarities can be obtained, not to the theory that there may be a cyclic association between sun-spots and the weather. If this association exists, yet no scrutiny of the sun can tell us more than we already know, and it will scarcely be pretended that new solar observatories could give us any better general idea of the progress of the great sun-spot period than we obtain from observatories already in existence, or, indeed, might obtain from the observations of a single amateur telescopicist.

I think it quite possible that, from the systematic study of terrestrial relations, the existence of a cyclic association between the great spot period and terrestrial phenomena may be demonstrated, instead of being merely surmised, as at present. By the way, it may be worth noting that a prediction relative to the coming winter [that of 1877-78] has been made on the faith of such association by Professor Piazzi Smyth. It runs as follows :—

“ Having recently computed the remaining observations of our earth-thermometers here, and prepared a new projection of all the observations from their beginning in 1837 to their calamitous close last year [1876]—results generally confirmatory of those arrived at in 1870 have been obtained, but with more pointed and immediate bearing on the weather now before us.

“ The chief features undoubtedly deducible for the past thirty-nine years, after eliminating the more seasonal effects of ordinary summer and winter, are :—

“ 1. Between 1837 and 1876 three great heat-waves, from without, struck this part of the earth, viz., the first in 1846·5, the second in 1858·0, and the third in 1868·7. And unless some very complete alteration in the weather is to take place, the next such visitation may be looked for in 1879·5, within limits of half a year each way.

“ 2. The next feature in magnitude and certainty is that the periods of minimum temperature, or cold, are not either in, or anywhere near, the middle time between the crests of those three chronologically identified heat-waves, but are comparatively close up to them *on either*

as we are so strongly assured, the most costly observations, the employment of the heaviest guns (and “great guns” are generally expensive), twenty or thirty years of time, and the closest scrutiny and research, to prove that sun-spots affect terrestrial relations in a definite manner, effects so extremely difficult to demonstrate cannot possibly be important enough to be worth predicting.

side, at a distance of about a year and a half, so that the next such cold-wave is due at the end of the present year [1877].

“This is, perhaps, not an agreeable prospect, especially if political agitators are at this time moving amongst the colliers, striving to persuade them to decrease the out-put of coal at every pit’s mouth. Being, therefore, quite willing, for the general good, to suppose myself mistaken, I beg to send you a first impression of plate 17 of the forthcoming volume of observations of this Royal Observatory, and shall be very happy if you can bring out from the measures recorded there any more comfortable view for the public at large.

“PIAZZI SMYTH,  
“Astronomer-Royal for Scotland.”

If this prediction shall be confirmed [this was written in autumn, 1877], it will afford an argument in favour of the existence of the cyclic relation suggested, but no argument for the endowment of solar research. Professor Smyth’s observations were not solar but terrestrial.

[The prediction was not confirmed, the winter of 1877-78 being, on the contrary, exceptionally mild.]

## *NEW WAYS OF MEASURING THE SUN'S DISTANCE.*

IT is strange that the problem of determining the sun's distance, which for many ages was regarded as altogether insoluble, and which even during later years had seemed fairly solvable in but one or two ways, should be found, on closer investigation, to admit of many methods of solution. If astronomers should only be as fortunate hereafter in dealing with the problem of determining the distances of the stars, as they have been with the question of the sun's distance, we may hope for knowledge respecting the structure of the universe such as even the Herschels despaired of our ever gaining. Yet this problem of determining star-distances does not seem more intractable, now, than the problem of measuring the sun's distance appeared only two centuries ago. If we rightly view the many methods devised for dealing with the easier task, we must admit that the more difficult—which, by the way, is in reality infinitely the more interesting—cannot be regarded as so utterly hopeless as, with our present methods and appliances, it appears to be. True, we know only the distances of two or three stars, approximately, and have means of forming a vague opinion about the distances of only a dozen others, or thereabouts, while at distances now immeasurable lie six thousand stars visible to the eye, and twenty millions within range of the telescope. Yet, in Galileo's time, men might have argued similarly against all hope of measuring the proportions of the solar

system. "We know only," they might have urged, "the distance of the moon, our immediate neighbour,—beyond her, at distances so great that hers, so far as we can judge, is by comparison almost as nothing, lie the Sun and Mercury, Venus and Mars; further away yet lie Jupiter and Saturn, and possibly other planets, not visible to the naked eye, but within range of that wonderful instrument, the telescope, which our Galileo and others are using so successfully. What hope can there be, when the exact measurement of the moon's distance has so fully taxed our powers of celestial measurement, that we can ever obtain exact information respecting the distances of the sun and planets? By what method is a problem so stupendous to be attacked?" Yet, within a few years of that time, Kepler had formed already a rough estimate of the distance of the sun; in 1639, young Horrocks pointed to a method which has since been successfully applied. Before the end of the seventeenth century Cassini and Flamsteed had approached the solution of the problem more nearly, while Halley had definitely formulated the method which bears his name. Long before the end of the eighteenth century it was certainly known that the sun's distance lies between 85 millions of miles and 98 millions (Kepler, Cassini, and Flamsteed had been unable to indicate any superior limit). And lastly, in our own time, half a score of methods, each subdivisible into several forms, have been applied to the solution of this fundamental problem of observational astronomy.

I propose now to sketch some new and very promising methods, which have been applied already with a degree of success arguing well for the prospects of future applications of the methods under more favourable conditions.

In the first place, let us very briefly consider the methods which had been before employed, in order that the proper position of the new methods may be more clearly recognized.

The plan obviously suggested at the outset for the solution of the problem was simply to deal with it as a problem of surveying. It was in such a manner that the moon's

distance had been found, and the only difficulty in applying the method to the sun or to any planet consisted in the delicacy of the observations required. The earth being the only surveying-ground available to astronomers in dealing with this problem (in dealing with the problem of the stars' distances they have a very much wider field of operations), it was necessary that a base-line should be measured on this globe of ours,—large enough compared with our small selves, but utterly insignificant compared with the dimensions of the solar system. The diameter of the earth being less than 8000 miles, the longest line which the observers could take for base scarcely exceeded 6000 miles; since observations of the same celestial object at opposite ends of a diameter necessarily imply that the object is in the horizon of *both* the observing stations (for precisely the same reason that two cords stretched from the ends of any diameter of a ball to a distant point touch the ball at those ends). But the sun's distance being some 92 millions of miles, a base of 6000 miles amounts to less than the 15,000th part of the distance to be measured. Conceive a surveyor endeavouring to determine the distance of a steeple or rock 15,000 feet, or nearly three miles, from him, with a base-line *one foot* in length, and you can conceive the task of astronomers who should attempt to apply the direct surveying method to determine the sun's distance,—at least, you have one of their difficulties strikingly illustrated, though a number of others remain which the illustration does not indicate. For, after all, a base one foot in length, though far too short, is a convenient one in many respects: the observer can pass from one end to the other without trouble—he looks at the distant object under almost exactly the same conditions from each end, and so forth. A base 6000 miles long for determining the sun's distance is too short in precisely the same degree, but it is assuredly not so convenient a base for the observer. A giant 36,000 miles high would find it as convenient as a surveyor six feet high would find a one foot base-line; but astronomers, as a rule, are less than 36,000 miles in height.

Accordingly the same observer cannot work at both ends of the base-line, and they have to send out expeditions to occupy each station. All the circumstances of temperature, atmosphere, personal observing qualities, etc., are unlike at the two ends of the base-line. The task of measuring the sun's distance directly is, in fact, at present beyond the power of observational astronomy, wonderfully through its methods have developed in accuracy.

We all know how, by observations of Venus in transit, the difficulty has been so far reduced that trustworthy results have been obtained. Such observations belong to the surveying method, only Venus's distance is made the object of measurement instead of the sun's. The sun serves simply as a sort of dial-plate, Venus's position while in transit across this celestial dial-plate being more easily measured than when she is at large upon the sky. The devices by which Halley and Delisle severally caused *time* to be the relation observed, instead of position, do not affect the general principle of the transit method. It remains dependent on the determination of position. Precisely as by the change of the *position* of the hands of a clock on the face we measure *time*, so by the transit method, as Halley and Delisle respectively suggested its use, we determine Venus's position on the sun's face, by observing the difference of the time she takes in crossing, or the difference of the time at which she begins to cross, or passes off, his face.

Besides the advantage of having a dial-face like the sun's, on which thus to determine positions, the transit method deals with Venus when at her nearest, or about 25 million miles from us, instead of the sun at his greater distance of from  $90\frac{1}{2}$  to  $93\frac{1}{2}$  millions of miles. Yet we do not get the entire advantage of this relative proximity of Venus. For the dial-face—the sun, that is—changes its position too—in less degree than Venus changes hers, but still so much as largely to reduce her seeming displacement. The sun being further away as 92 to 25, is less displaced as 25 to 92. Venus's displacement is thus diminished by  $\frac{25}{92}$ nds of its full.

amount, leaving only  $\frac{67}{92}$ nds. Practically, then, the advantage of observing Venus, so far as distance is concerned, is the same as though, instead of being at a distance of only 25 million miles, her distance were greater as 92 to 67, giving as her effective distance when in transit some 34,300,000 miles.

All the methods of observing Venus in transit are affected in *this* respect. Astronomers were not content during the recent transit to use Halley's and Delisle's two time methods (which may be conveniently called the duration method and the epoch method), but endeavoured to determine the position of Venus on the sun's face directly, both by observation and by photography. The heliometer was the instrument specially used for the former purpose; and as, in one of the new methods to be presently described, this is the most effective of all available instruments, a few words as to its construction will not be out of place.

The heliometer, then, is a telescope whose object-glass (that is, the large glass at the end towards the object observed) is divided into two halves along a diameter. When these two halves are exactly together—that is, in the position they had before the glass was divided—of course they show any object to which they may be directed precisely as they would have done before the glass was cut. But if, without separating the straight edges of the two semicircular glasses, one be made to slide along the other, the images formed by the two no longer coincide.\* Thus, if we are looking at the sun we see two overlapping discs, and by continuing to turn the screw or other mechanism which carries our half-circular glass past the other, the disc-images

\* The reader unfamiliar with the principles of the telescope may require to be told that in the ordinary telescope each part of the object-glass forms a complete image of the object examined. If, when using an opera-glass (one barrel), a portion of the large glass be covered, a portion of what had before been visible is concealed. But this is not the case with a telescope of the ordinary construction. All that happens when a portion of the object-glass is covered is that the object appears in some degree less fully illuminated.

of the sun may be brought entirely clear of each other. Then we have two suns in the same field of view, seemingly in contact, or nearly so. Now, if we have some means of determining how far the movable half-glass has been carried past the other to bring the two discs into apparently exact contact, we have, in point of fact, a measure of the sun's apparent diameter. We can improve this estimate by carrying back the movable glass till the images coincide again, then further back till they separate the other way and finally are brought into contact on that side. The entire range, from contact on one side to contact on the other side, gives twice the entire angular span of the sun's diameter; and the half of this is more likely to be the true measure of the diameter, than the range from coincident images to contact either way, simply because instrumental errors are likely to be more evenly distributed over the double motion than over the movement on either side of the central position. The heliometer derived its name—which signifies sun-measurer—from this particular application of the instrument.

It is easily seen how the heliometer was made available in determining the position of Venus at any instant during transit. The observer could note what displacement of the two half-glasses was necessary to bring the black disc of Venus on one image of the sun to the edge of the other image, first touching on the inside and then on the outside. Then, reversing the motion, he could carry her disc to the opposite edge of the other image of the sun, first touching on the inside and then on the outside. Lord Lindsay's private expedition—one of the most munificent and also one of the most laborious contributions to astronomy ever made—was the only English expedition which employed the heliometer, none of our public observatories possessing such an instrument, and official astronomers being unwilling to ask Government to provide instruments so costly. The Germans, however, and the Russians employed the heliometer very effectively.

Next in order of proximity, for the employment of the

direct surveying method, is the planet Mars when he comes into opposition (or on the same line as the earth and sun) in the order

Sun \_\_\_\_\_ Earth \_\_\_\_\_ Mars,

at a favourable part of his considerably eccentric orbit. His distance then may be as small as  $34\frac{1}{2}$  millions of miles; and we have in his case to make no reduction for the displacement of the background on which his place is to be determined. That background is the star sphere, his place being measured from that of stars near which his apparent path on the heavens carries him; and the stars are so remote that the displacement due to a distance of six or seven thousand miles between two observers on the earth is to all intents and purposes nothing. The entire span of the earth's orbit round the sun, though amounting to 184 millions of miles, is a mere point as seen from all save ten or twelve stars; how utterly evanescent, then, the span of the earth's globe—less than the 23,000th part of her orbital range! Thus the entire displacement of Mars due to the distance separating the terrestrial observers comes into effect. So that, in comparing the observation of Mars in a favourable opposition with that of Venus in transit, we may fairly say that, so far as surveying considerations are concerned, the two planets are equally well suited for the astronomer's purpose. Venus's less distance of 25 millions of miles is effectively increased to  $34\frac{1}{2}$  millions by the displacement of the solar background on which we see her when in transit; while Mars's distance of about  $34\frac{1}{2}$  millions of miles remains effectively the same when we measure his displacement from neighbouring fixed stars.

But in many respects Mars is superior to Venus for the purpose of determining the sun's distance. Venus can only be observed at her nearest when in transit, and transit lasts but a few hours. Mars can be observed night after night for a fortnight or so, during which his distance still remains near enough to the least or opposition distance. Again,

Venus being observed on the sun, all the disturbing influences due to the sun's heat are at work in rendering the observation difficult. The air between us and the sun at such a time is disturbed by undulations due in no small degree to the sun's action. It is true that we have not, in the case of Mars, any means of substituting time measures or time determinations for measures of position, as we have in Venus's case, both with Halley's and Delisle's methods. But to say the truth, the advantage of substituting these time observations has not proved so great as was expected. Venus's unfortunate deformity of figure when crossing the sun's edge renders the determination of the exact moments of her entry on the sun's face and of her departure from it by no means so trustworthy as astronomers could wish. On the whole, Mars would probably have the advantage even without that point in his favour which has now to be indicated.

Two methods of observing Mars for determining the sun's distance are available, both of which, as they can be employed in applying one of the new methods, may conveniently be described at this point.

An observer far to the north of the earth's equator sees Mars at midnight, when the planet is in opposition, displaced somewhat to the south of his true position—that is, of the position he would have as supposed to be seen from the centre of the earth. On the other hand, an observer far to the south of the equator sees Mars displaced somewhat to the north of his true position. The difference may be compared to different views of a distant steeple (projected, let us suppose, against a much more remote hill), from the uppermost and lowermost windows of a house corresponding to the northerly and southerly stations on the earth, and from a window on the middle story corresponding to a view of Mars from the earth's centre. By ascertaining the displacement of the two views of Mars obtained from a station far to the north and another station far to the south, the astronomer can infer the distance of the planet, and thence the dimensions of the solar system. The displacement is

determinable by noticing Mars's position with respect to stars which chance to be close to him. For this purpose the heliometer is specially suitable, because, having first a view of Mars and some companion stars as they actually are placed, the observer can, by suitably displacing the movable half-glass, bring the star into apparent contact with the planet, first on one side of its disc, and then on the other side—the mean of the two resulting measures giving, of course, the distance between the star and the centre of the disc.

This method requires that there shall be two observers, one at a northern station, as Greenwich, or Paris, or Washington, the other at a southern station, as Cape Town, Cordoba, or Melbourne. The base-line is practically a north-and-south line; for though the two stations may not lie in the same, or nearly the same, longitude, the displacement determined is in reality that due to their difference of latitude only, a correction being made for their difference of longitude.

The other method depends, not on displacement of two observers north and south, or difference of latitude, but on displacement east and west. Moreover, it does not require that there shall be two observers at stations far apart, but uses the observations made at one and the same stations at different times. The earth, by turning on her axis, carries the observer from the west to the east of an imaginary line joining the earth's centre and the centre of Mars. When on the west of that line, or in the early evening, he sees Mars displaced towards the east of the planet's true position. After nine or ten hours the observer is carried as far to the east of that line, and sees Mars displaced towards the west of his true position. Of course Mars has moved in the interval. He is, in fact, in the midst of his retrograde career. But the astronomer knows perfectly well how to take that motion into account. Thus, by observing the two displacements, or the total displacement of Mars from east to west on account of the earth's rotation, one and the same

observer can, in the course of a single favourable night, determine the sun's distance. And in passing, it may be remarked that this is the only general method of which so much can be said. By some of the others an astronomer can, indeed, estimate the sun's distance without leaving his observatory—at least, theoretically he can do so. But many years of observation would be required before he would have materials for achieving this result. On the other hand, one good pair of observations of Mars, in the evening and in the morning, from a station near the equator, would give a very fair measure of the sun's distance. The reason why the station should be near the equator will be manifest, if we consider that at the poles there would be no displacement due to rotation ; at the equator the observer would be carried round a circle some twenty-five thousand miles in circumference ; and the nearer his place to the equator the larger the circle in which he would be carried, and (*cæteris paribus*) the greater the evening and morning displacement of the planet.

Both these methods have been successfully applied to the problem of determining the sun's distance, and both have recently been applied afresh under circumstances affording exceptionally good prospects of success, though as yet the results are not known.

It is, however, when we leave the direct surveying method to which both the observations of Venus in transit and Mars in opposition belong (in all their varieties), that the most remarkable, and, one may say, unexpected methods of determining the sun's distance present themselves. Were not my subject a wide one, I would willingly descant at length on the marvellous ingenuity with which astronomers have availed themselves of every point of vantage whence they might measure the solar system. But, as matters actually stand, I must be content to sketch these other methods very roughly, only indicating their characteristic features.

One of them is in some sense related to the method by actual survey, only it takes advantage, not of the earth's dimensions, but of the dimensions of her orbit round the

common centre of gravity of herself and the moon. This orbit has a diameter of about six thousand miles ; and as the earth travels round it, speeding swiftly onwards all the time in her path round the sun, the effect is the same as though the sun, in his apparent circuit round the earth, were constantly circling once in a lunar month around a small subordinate orbit of precisely the same size and shape as that small orbit in which the earth circuits round the moon's centre of gravity. He appears then sometimes displaced about 3000 miles on one side, sometimes about 3000 miles on the other side of the place which he would have if our earth were not thus perturbed by the moon. But astronomers can note each day where he is, and thus learn by how much he seems displaced from his mean position. Knowing that his greatest displacement corresponds to so many miles exactly, and noting what it seems to be, they learn, in fact, how large a span of so many miles (about 3000) looks at the sun's distance. Thus they learn the sun's distance precisely as a rifleman learns the distance of a line of soldiers when he has ascertained their apparent size—for only at a certain distance can an object of known size have a certain apparent size.

The moon comes in, in another way, to determine the sun's distance for us. We know how far away she is from the earth, and how much, therefore, she approaches the sun when new, and recedes from him when full. Calling this distance, roughly, a 390th part of the sun's, her distance from him when new, her mean distance, and her distance from him when full, are as the numbers 389, 390, 391. Now, these numbers do not quite form a continued proportion, though they do so very nearly (for 389 is to 390 as 390 to  $391\frac{1}{100}$ ). If they were in exact proportion, the sun's disturbing influence on the moon when she is at her nearest would be exactly equal to his disturbing influence on the moon when at her furthest from him—or generally, the moon would be exactly as much disturbed (on the average) in that half of her path which lies nearer to the sun as in that half which lies further from him. As matters are, there is a slight

difference. Astronomers can measure this difference ; and measuring it, they can ascertain what the actual numbers are for which I have roughly given the numbers 389, 390, and 391 ; in other words, they can ascertain in what degree the sun's distance exceeds the moon's. This is equivalent to determining the sun's distance, since the moon's is already known.

Another way of measuring the sun's distance has been "favoured" by Jupiter and his family of satellites. Few would have thought, when Römer first explained the delay which occurs in the eclipse of these moons while Jupiter is further from us than his mean distance, that that explanation would lead up to a determination of the sun's distance. But so it happened. Römer showed that the delay is not in the recurrence of the eclipses, but in the arrival of the news of these events. From the observed time required by light to traverse the extra distance when Jupiter is nearly at his furthest from us, the time in which light crosses the distance separating us from the sun is deduced ; whence, if that distance has been rightly determined, the velocity of light can be inferred. If this velocity is directly measured in any way, and found not to be what had been deduced from the adopted measure of the sun's distance, the inference is that the sun's distance has been incorrectly determined. Or, to put the matter in another way, we know exactly how many minutes and seconds light takes in travelling to us from the sun ; if, therefore, we can find out how fast light travels we know how far away the sun is.

But who could hope to measure a velocity approaching 200,000 miles in a second ? At a first view the task seems hopeless. Wheatstone, however, showed how it might be accomplished, measuring by his method the yet greater velocity of freely conducted electricity. Foucault and Fizeau measured the velocity of light ; and recently Cornu has made more exact measurements. Knowing, then, how many miles light travels in a second, and in how many seconds it comes to us from the sun, we know the sun's distance.

The first of the methods which I here describe as new methods must next be considered. It is one which Leverrier regards as the method of the future. In fact, so highly does he esteem it, that, on its account, he may almost be said to have refused to sanction in any way the French expeditions for observing the transit of Venus in 1874.

The members of the sun's family perturb each other's motions in a degree corresponding with their relative mass, compared with each other and with the sun. Now, it can be shown (the proof would be unsuitable to these pages,\* but I have given it in my treatise on "The Sun") that no change in our estimate of the sun's distance affects our estimate of his mean density as compared with the earth's. His substance has a mean density equal to one-fourth of the earth's, whether he be 90 millions or 95 millions of miles from us, or indeed whether he were ten millions or a million million miles from us (supposing for a moment our measures did not indicate his real distance more closely). We should still deduce from

\* It may be briefly sketched, perhaps, in a note. The force necessary to draw the earth inwards in such sort as to make her follow her actual course is proportional to (i) the square of her velocity directly, and (ii) her distance from the sun inversely. If we increase our estimate of the earth's distance from the sun, we, in the same degree, increase our estimate of her orbital velocity. The square of this velocity then increases as the square of the estimated distance; and therefore, the estimated force sunwards is increased as the square of the distance on account of (i), and diminished as the distance on account of (ii), and is, therefore, on the whole, increased as the distance. That is, we now regard the sun's action as greater at this greater distance, and in the same degree that the distance is greater; whereas, if it had been what we before supposed it, it would be less at the greater distance as the square of the distance (attraction varying inversely as the square of the distance). Being greater as the distance, instead of less as the square of the distance, it follows that our estimate of the sun's absolute force is now greater as the cube of the distance. Similarly, if we had diminished our estimate of the sun's distance, we should have diminished our estimate of his absolute power (or mass) as the cube of the distance. But our estimate of the sun's volume is also proportional to the cube of his estimated distance. Hence our estimate of his mass varies as our estimate of his volume; or, our estimate of his mean density is constant.

calculation the same unvarying estimate of his mean density. It follows that the nearer any estimate of his distance places him, and therefore the smaller it makes his estimated volume, the smaller also it makes his estimated mass, and in precisely the same degree. The same is true of the planets also. We determine Jupiter's mass, for example (at least, this is the simplest way), by noting how he swerves his moons at their respective (estimated) distances. If we diminish our estimate of their distances, we diminish at the same time our estimate of Jupiter's attractive power, and in such degree, it may be shown (see note), as precisely to correspond with our changed estimate of his size, leaving our estimate of his mean density unaltered. And the same is true for all methods of determining Jupiter's mass. Suppose, then, that, adopting a certain estimate of the scale of the solar system, we find that the resulting estimate of the masses of the planets and of the sun, *as compared with the earth's mass*, from their observed attractive influences on bodies circling around them or passing near them, accords with their estimated perturbing action as compared with the earth's,—then we should infer that our estimate of the sun's distance or of the scale of the solar system was correct. But suppose it appeared, on the contrary, that the earth took a larger or a smaller part in perturbing the planetary system than, according to our estimate of her relative mass, she should do,—then we should infer that the masses of the other members of the system had been overrated or underrated; or, in other words, that the scale of the solar system had been overrated or underrated respectively. Thus we should be able to introduce a correction into our estimate of the sun's distance.

Such is the principle of the method by which Leverrier showed that in the astronomy of the future the scale of the solar system may be very exactly determined. Of course, the problem is a most delicate one. The earth plays, in truth, but a small part in perturbing the planetary system, and her influence can only be distinguished satisfactorily (at present,

at any rate) in the case of the nearer members of the solar family. Yet the method is one which, unlike others, will have an accumulative accuracy, the discrepancies which are to test the result growing larger as time proceeds. The method has already been to some extent successful. It was, in fact, by observing that the motions of Mercury are not such as can be satisfactorily explained by the perturbations of the earth and Venus according to the estimate of relative masses deducible from the lately discarded value of the sun's distance, that Leverrier first set astronomers on the track of the error affecting that value. He was certainly justified in entertaining a strong hope that hereafter this method will be exceedingly effective.

We come next to a method which promises to be more quickly if not more effectively available.

Venus and Mars approach the orbit of our earth more closely than any other planets, Venus being our nearest neighbour on the one side, and Mars on the other. Looking beyond Venus, we find only Mercury (and the mythical Vulcan), and Mercury can give no useful information respecting the sun's distance. He could scarcely do so even if we could measure his position among the stars when he is at his nearest, as we can that of Mars ; but as he can only then be fairly seen when he transits the sun's face, and as the sun is nearly as much displaced as Mercury by change in the observer's station, the difference between the two displacements is utterly insufficient for accurate measurement. But, when we look beyond the orbit of Mars, we find certain bodies which are well worth considering in connection with the problem of determining the sun's distance. I refer to the asteroids, the ring of small planets travelling between the paths of Mars and Jupiter, but nearer (on the whole \*) to the path of Mars than to that of Jupiter.

\* Only very recently an asteroid, Hilda (153rd in order of detection), has been discovered which travels very much nearer to the path of Jupiter than to that of Mars—a solitary instance in that respect. Its distance (the earth's distance being represented by unity), is 3·95.

The asteroids present several important advantages over even Mars and Venus.

Of course, none of the asteroids approach so near to the earth as Mars at his nearest. His least distance from the sun being about 127 million miles, and the earth's mean distance about 92 millions, with a range of about a million and a half on either side, owing to the eccentricity of her orbit, it follows that he *may* be as near as some 35 million miles (rather less in reality) from the earth when the sun, earth, and Mars are nearly in a straight line and in that order. The least distance of any asteroid from the sun amounts to about 167 million miles, so that their least distance from the earth cannot at any time be less than about 73,500,000 miles, even if the earth's greatest distance from the sun corresponded with the least distance of one of these closely approaching asteroids. This, by the way, is not very far from being the case with the asteroid Ariadne, which comes within about 169 million miles of the sun at her nearest, her place of nearest approach being almost exactly in the same direction from the sun as the earth's place of greatest recession, reached about the end of June. So that, whenever it so chances that Ariadne comes into opposition in June, or that the sun, earth, and Ariadne are thus placed—

Sun                    Earth                    Ariadne,

Ariadne will be but about 75,500,000 miles from the earth. Probably no asteroid will ever be discovered which approaches the earth much more nearly than this; and this approach, be it noticed, is not one which can occur in the case of Ariadne except at very long intervals.

But though we may consider 80 millions of miles as a fair average distance at which a few of the most closely approaching asteroids may be observed, and though this

Jupiter's being 5·20, and Mars's 1·52; its period falls short of 8 years by only two months, the average period of the asteroidal family being only about 4½ years. Five others, Cybele, Freia, Sylvia, Camilla, and Hermione, travel rather nearer to Jupiter than to Mars; but the remaining 166 travel nearer to Mars, and most of them much nearer.

distance seems very great by comparison with Mars's occasional opposition distance of 35 million miles, yet there are two points in which asteroids have the advantage over Mars. First, they are many, and several among them can be observed under favourable circumstances ; and in the multitude of observations there is safety. In the second place, which is the great and characteristic good quality of this method of determining the sun's distance, they do not present a disc, like the planet Mars, but a small star-like point. When we consider the qualities of the heliometric method of measuring the apparent distance between celestial objects, the advantage of points of light over discs will be obvious. If we are measuring the apparent distance between Mars and a star, we must, by shifting the movable object-glass, bring the star's image into apparent contact with the disc-image of Mars, first on one side and then on the other, taking the mean for the distance between the centres. Whereas, when we determine the distance between a star and an asteroid, we have to bring two star-like points (one a star, the other the asteroid) into apparent coincidence. We can do this in two ways, making the result so much the more accurate. For consider what we have in the field of view when the two halves of the object-glass coincide. There is the asteroid and close by there is the star whose distance we seek to determine in order to ascertain the position of the asteroid on the celestial sphere. When the movable half is shifted, the two images of star and asteroid separate ; and by an adjustment they can be made to separate along the line connecting them. Suppose, then, we first make the movable image of the asteroid travel away from the fixed image (meaning by movable and fixed images, respectively, those given by the movable and fixed halves of the object-glass), towards the fixed image of the star—the two points, like images, being brought into coincidence, we have the measure of the distance between star and asteroid. Now reverse the movement, carrying back the movable images of the asteroid and star till they coincide again with their

fixed images. This movement gives us a second measure of the distance, which, however, may be regarded as only a reversed repetition of the preceding. But now, carrying on the reverse motion, the moving images of star and asteroid separate from their respective fixed images, the moving image of the star drawing near to the fixed image of the asteroid and eventually coinciding with it. Here we have a third measure of the distance, which is independent of the two former. Reversing the motion, and carrying the moving images to coincidence with the fixed images, we have a fourth measure, which is simply the third reversed. These four measures will give a far more satisfactory determination of the true apparent distance between the star and the asteroid than can, under any circumstances, be obtained in the case of Mars and a star. Of course, a much more exact determination is required to give satisfactory measures of the asteroid's real distance from the earth in miles, for a much smaller error would vitiate the estimate of the asteroid's distance than would vitiate to the same degree the estimate of Mars's distance : for the apparent displacements of the asteroid as seen either from Northern and Southern stations, or from stations east and west of the meridian, are very much less than in the case of Mars, owing to his great proximity. But, on the whole, there are reasons for believing that the advantage derived from the nearness of Mars is almost entirely counterbalanced by the advantage derived from the neatness of the asteroid's image. And the number of asteroids, with the consequent power of repeating such measurements many times for each occasion on which Mars has been thus observed, seem to make the asteroids—so long regarded as very unimportant members of the solar system—the bodies from which, after all, we shall gain our best estimate of the sun's distance ; that is, of the scale of the solar system.

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Since the above pages were written, the results deduced from the observations made by the British expeditions for

observing the transit of December 9, 1874, have been announced by the Astronomer Royal. It should be premised that they are not the results deducible from the entire series of British observations, for many of them can only be used effectively in combination with observations made by other nations. For instance, the British observations of the duration of the transit as seen from Southern stations are only useful when compared with observations of the duration of the transit as seen from Northern stations, and no British observations of this kind were taken at Northern stations, or could be taken at any of the British Northern stations except one, where chief reliance was placed on photographic methods. The only British results as yet "worked up" are those which are of themselves sufficient, theoretically, to indicate the sun's distance, *viz.*, those which indicated the epochs of the commencement of transit as seen from Northern and Southern stations, and those which indicated the epochs of the end of transit as seen from such stations. The Northern and Southern epochs of commencement compared together suffice *of themselves* to indicate the sun's distance; so also do the epochs of the end of transit suffice *of themselves* for that purpose. Such observations belong to the Delislean method, which was the subject of so much controversy for two or three years before the transit took place. Originally it had been supposed that only observations by that method were available, and the British plans were formed upon that assumption. When it was shown that this assumption was altogether erroneous, there was scarcely time to modify the British plans so that of themselves they might provide for the other or Halleyan method. But the Southern stations which were suitable for that method were strengthened; and as other nations, especially America and Russia, occupied large numbers of Northern stations, the Halleyan method was, in point of fact, effectually provided for—a fortunate circumstance, as will presently be seen.

The British operations, then, thus far dealt with, were based on Delisle's method; and as they were carried out

with great zeal and completeness, we may consider that the result affords an excellent test of the qualities of this method, and may supply a satisfactory answer to the questions which were under discussion in 1872-74. Sir George Airy, indeed, considers that the zeal and completeness with which the British operations were carried out suffice to set the result obtained from them above all others. But this opinion is based rather on personal than on strictly scientific grounds. It appears to me that the questions to be primarily decided are whether the results are in satisfactory agreement (i) *inter se* and (ii) with the general tenor of former researches. In other words, while the Astronomer Royal considers that the method and the manner of its application must be considered so satisfactory that the results are to be accepted unquestionably, it appears to me that the results must be carefully questioned (as it were) to see whether the method, and the observations by it, are satisfactory. In the first place, the result obtained from Northern and Southern observations of the commencement ought to agree closely with the result obtained from Northern and Southern observations of the end of transit. Unfortunately, they differ rather widely. The sun's distance by the former observations comes out about one million miles greater than the distance determined by the latter observations.

This should be decisive, one would suppose. But it is not all. The mean of the entire series of observations by Delisle's method comes out nearly one million miles greater than the mean deduced by Professor Newcomb from many entire series of observations by six different methods, all of which may fairly be regarded as equal in value to Delisle's, while three are regarded by most astronomers as unquestionably superior to it. Newcomb considers the probable limits of error in his evaluation from so many combined series of observations to be about 100,000 miles. Sir G. Airy will allow no wider limits of error for the result of the one series his observers have obtained than 200,000 miles. Thus the greatest value admitted by Newcomb falls short

of the least value admitted by Sir G. Airy, by nearly 700,000 miles.

The obvious significance of this result should be, one would suppose, that Delisle's method is not quite so effective as Sir G. Airy supposed ; and the wide discordance between the several results, of which the result thus deduced is the mean, should prove this, one would imagine, beyond all possibility of question. The Astronomer Royal thinks differently, however. In his opinion, the wide difference between his result and the mean of all the most valued results by other astronomers, indicates the superiority of Delisle's method, not its inadequacy to the purpose for which it has been employed.

Time will shortly decide which of these views is correct ; but, for my own part, I do not hesitate to express my own conviction that the sun's distance lies very near the limits indicated by Newcomb, and therefore is several hundred thousand miles less than the minimum distance allowed by the recently announced results.

## *DRIFTING LIGHT-WAVES.*

THE method of measuring the motion of very swiftly travelling bodies by noting changes in the light-waves which reach us from them—one of the most remarkable methods of observation ever yet devised by man—has recently been placed upon its trial, so to speak ; with results exceedingly satisfactory to the students of science who had accepted the facts established by it. The method will not be unfamiliar to many of my readers. The principle involved was first noted by M. Doppler, but not in a form which promised any useful results. The method actually applied appears to have occurred simultaneously to several persons, as well theorists as observers. Thus Secchi claimed in March, 1868, to have applied it, though unsuccessfully ; Huggins in April, 1868, described his successful use of the method. I myself, wholly unaware that either of these observers was endeavouring to measure celestial motions by its means, described the method, in words which I shall presently quote, in the number of *Fraser's Magazine* for January, 1868, two months before the earliest enunciation of its nature by the physicists just named.

It will be well briefly to describe the principle of this interesting method, before considering the attack to which it has been recently subjected, and its triumphant acquittal from defects charged against it. This brief description will not only be useful to those readers who chance not to be acquainted with the method, but may serve to remove objections which suggest themselves, I notice, to many who

have had the principle of the method imperfectly explained to them.

Light travels from every self-luminous body in waves which sweep through the ether of space at the rate of 185,000 miles per second. The whole of that region of space over which astronomers have extended their survey, and doubtless a region many millions of millions of times more extended, may be compared to a wave-tossed sea, only that instead of a wave-tossed surface, there is wave-tossed space. At every point, through every point, along every line, athwart every line, myriads of light-waves are at all times rushing with the inconceivable velocity just mentioned.

It is from such waves that we have learned all we know about the universe outside our own earth. They bring to our shores news from other worlds, though the news is not always easy to decipher.

Now, seeing that we are thus immersed in an ocean, athwart which infinite series of waves are continually rushing, and moreover that we ourselves, and every one of the bodies whence the waves proceed either directly or after reflection, are travelling with enormous velocity through this ocean, the idea naturally presents itself that we may learn something about these motions (as well as about the bodies themselves whence they proceed), by studying the aspect of the waves which flow in upon us in all directions.

Suppose a strong swimmer who knew that, were he at rest, a certain series of waves would cross him at a particular rate—ten, for instance, in a minute—were to notice that when he was swimming directly facing them, eleven passed him in a minute: he would be able at once to compare his rate of swimming with the rate of the waves' motion. He would know that while ten waves had passed him on account of the waves' motion, he had by his own motion caused yet another wave to pass him, or in other words, had traversed the distance from one wave-crest to the next. Thus he would know that his rate was one-tenth that of the waves. Similarly if, travelling the same way as the waves,

he found that only nine passed him in a minute, instead of ten.

Again, it is not difficult to see that if an observer were at rest, and a body in the water, which by certain motions produced waves, were approaching or receding from the observer, the waves would come in faster in the former case, slower in the latter, than if the body were at rest. Suppose, for instance, that some machinery at the bows of a ship raised waves which, if the ship were at rest, would travel along at the rate of ten a minute past the observer's station. Then clearly, if the ship approached him, each successive wave would have a shorter distance to travel, and so would reach him sooner than it otherwise would have done. Suppose, for instance, the ship travelled one-tenth as fast as the waves, and consider ten waves proceeding from her bows—the first would have to travel a certain distance before reaching the observer; the tenth, starting a minute later, instead of having to travel the same distance, would have to travel this distance diminished by the space over over which the ship had passed in one minute (which the wave itself passes over in the tenth of a minute); instead, then, of reaching the observer one minute after the other, it would reach him nine-tenths of a minute after the first. Thus it would seem to him as though the waves were coming in faster than when the ship was at rest, in the proportion of ten to nine, though in reality they would be travelling at the same rate as before, only arriving in quicker succession, because of the continual shortening of the distance they had to travel, on account of the ship's approach. If he knew precisely how fast they *would* arrive if the ship were at rest, and determined precisely how fast they *did* arrive, he would be able to determine at once the rate of the ship's approach, at least the proportion between her rate and the rate of the waves' motion. Similarly if, owing to the ship's recession, the apparent rate of the waves' motion were reduced, it is obvious that the actual change in the wave motion would not be a difference

of rate ; but, in the case of the approaching ship, the breadth from crest to crest would be reduced, while in the case of a receding ship the distance from crest to crest would be increased.

If the above explanation should still seem to require closer attention than the general reader may be disposed to give, the following, suggested by a friend of mine—a very skilful mathematician—will be found still simpler : Suppose a stream to flow quite uniformly, and that at one place on its banks an observer is stationed, while at another higher up a person throws corks into the water at regular intervals, say ten corks per minute ; then these will float down and pass the other observer, wherever he may be, at the rate of ten per minute, *if* the cork-thrower is at rest. But if he saunters either up-stream or down-stream, the corks will no longer float past the other at the exact rate of ten per minute. If the thrower is sauntering down-stream, then, between throwing any cork and the next, he has walked a certain way down, and the tenth cork, instead of having to travel the same distance as the first before reaching the observer, has a shorter distance to travel, and so reaches that observer sooner. Or in fact, which some may find easier to see, this cork will be nearer to the first cork than it would have been if the thrower had remained still. The corks will lie at equal distances from each other, but these equal distances will be less than they would have been if the observer had been at rest. If, on the contrary, the cork-thrower saunters up-stream, the corks will be somewhat further apart than if he had remained at rest. And supposing the observer to know beforehand that the corks would be thrown in at the rate of ten a minute, he would know, if they passed him at a greater rate than ten a minute (or, in other words, at a less distance from each other than the stream traversed in the tenth of a minute), that the cork-thrower was travelling down-stream or approaching him ; whereas, if fewer than ten a minute passed him, he would know that the cork-thrower was travelling away from him, or up-stream. But also, if the

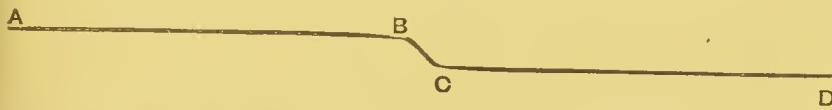
cork-thrower were at rest, and the observer moved up-stream—that is, towards him—the corks would pass him at a greater rate than ten a minute; whereas, if the observer were travelling down-stream, or from the thrower, they would pass him at a slower rate. If both were moving, it is easily seen that if their movement brought them nearer together, the number of corks passing the observer per minute would be increased, whereas if their movements set them further apart, the number passing him per minute would be diminished.

These illustrations, derived from the motions of water, suffice in reality for our purpose. The waves which are emitted by luminous bodies in space travel onwards like the water-waves or the corks of the preceding illustrations. If the body which emits them is rapidly approaching us, the waves are set closer together or narrowed; whereas, if the body is receding, they are thrown further apart or broadened. And if we can in any way recognize such narrowing or broadening of the light-waves, we know just as certainly that the source of light is approaching us or receding from us (as the case may be) as our observer in the second illustration would know from the distance between the corks whether his friend, the cork-thrower, was drawing near to him or travelling away from him.

But it may be convenient to give another illustration, drawn from waves, which, like those of light, are not themselves discernible by our senses—I refer to those aerial waves of compression and rarefaction which produce what we call sound. These waves are not only in this respect better suited than water-waves to illustrate our subject, but also because they travel in all directions through aerial space, not merely along a surface. The waves which produce a certain note, that is, which excite in our minds, through the auditory nerve, the impression corresponding to a certain tone, have a definite length. So long as the observer, and a source of sound vibrating in one particular period, remain both in the same place, the note is unchanged in tone, though

it may grow louder or fainter according as the vibrations increase or diminish in amplitude. But if the source of sound is approaching the hearer, the waves are thrown closer together and the sound is rendered more acute (the longer waves giving the deeper sound); and, on the other hand, if the source of sound is receding from the hearer, the waves are thrown further apart and the sound is rendered graver. The *rationale* of these changes is precisely the same as that of the changes described in the preceding illustrations. It might, perhaps, appear that in so saying we were dismissing the illustration from sound, at least as an independent one, because we are explaining the illustration by preceding illustrations. But in reality, while there is absolutely nothing new to be said respecting the increase and diminution of distances (as between the waves and corks of the preceding illustration), the illustration from sound has the immense advantage of admitting readily of experimental tests. It is necessary only that the rate of approach or recession should bear an appreciable proportion to the rate at which sound travels. For waves are shortened or lengthened by approach or recession by an amount which bears to the entire length of the wave the same proportion which the rate of approach or recession bears to the rate of the wave's advance. Now it is not very difficult to obtain rates of approach or recession fairly comparable with the velocity of sound—about 364 yards per second. An express train at full speed travels, let us say, about 1800 yards per minute, or 30 yards per second. Such a velocity would suffice to reduce all the sound-waves proceeding from a bell or whistle upon the engine, by about one-twelfth part, for an observer at rest on a station platform approached by the engine. On the contrary, after the engine had passed him, the sound-waves proceeding from the same bell or whistle would be lengthened by one-twelfth. The difference between the two tones would be almost exactly three semitones. If the hearer, instead of being on a platform, were in a train carried past the other at the same rate, the difference between the tone of the bell in approaching

and its tone in receding would be about three tones. It would not be at all difficult so to arrange matters, that while two bells were sounding the same note—*Mi*, let us say—one bell on one engine the other on the other, a traveller by one should hear his own engine's bell, the bell of the approaching engine, and the bell of the same engine receding, as the three notes—*Do*—*Mi*—*Sol*, whose wave-lengths are as the numbers 15, 12, and 10. We have here differences very easily to be recognized even by those who are not musicians. Every one who travels much by train must have noticed how the tone of a whistle changes as the engine sounding it travels past. The change is not quite sharp, but very rapid, because the other engine does not approach with a certain velocity up to a definite moment and then recede with the same velocity. It could only do this by rushing through the hearer, which would render the experiment theoretically more exact but practically unsatisfactory. As it rushes past instead of through him, there is a brief time during which the rate of approach is rapidly being reduced to nothing, followed by a similarly brief time during which the rate of recession gradually increases from nothing up to the actual rate of the engines' velocities added together.\* The change of tone may be thus illustrated :—



A B representing the sound of the approaching whistle, B C representing the rapid degradation of sound as the engine rushes close past the hearer, and C D representing the sound of the receding whistle. When a bell is sounded on the

\* Even this statement is not mathematically exact. If the rails are straight and parallel, the ratio of approach and recession of an engine on one line, towards or from an engine on the other, is never quite equal to the engines' velocities added together; but the difference amounts practically to nothing, except when the engines are near each other.

engine, as in America, the effect is better recognized, as I had repeated occasion to notice during my travels in that country. Probably this is because the tone of a bell is in any case much more clearly recognized than the tone of a railway whistle. The change of tone as a clanging bell is carried swiftly past (by the combined motions of both trains) is not at all of such a nature as to require close attention for its detection.

However, the apparent variation of sound produced by rapid approach or recession has been tested by exact experiments. On a railway uniting Utrecht and Maarsen "were placed," the late Professor Nichol wrote, "at intervals of something upwards of a thousand yards, three groups of musicians, who remained motionless during the requisite period. Another musician on the railway sounded at intervals one uniform note; and its effects on the ears of the stationary musicians have been fully published. From these, certainly—from the recorded changes between grave and the more acute, and *vice versa*—confirming, even *numerically*, what the relative velocities might have enabled one to predict, it appears justifiable to conclude that the general theory is correct; and that the note of any sound may be greatly modified, if not wholly changed, by the velocity of the individual hearing it," or, he should have added, by the velocity of the source of sound: perhaps more correct than either, is the statement that the note may be altered by the approach or recession of the source of sound, whether that be caused by the motion of the sounding body, or of the hearer himself, or of both.

It is difficult, indeed, to understand how doubt can exist in the mind of any one competent to form an opinion on the matter, though, as we shall presently see, some students of science and one or two mathematicians have raised doubts as to the validity of the reasoning by which it is shown that a change should occur. That the reasoning is sound cannot, in reality, be questioned, and after careful examination of the arguments urged against it by one or two mathematicians,

I can form no other opinion than that these arguments amount really but to an expression of inability to understand the matter. This may seem astonishing, but is explained when we remember that some mathematicians, by devoting their attention too particularly to special departments, lose, to a surprising degree, the power of dealing with subjects (even mathematical ones) outside their department. Apart from the soundness of the reasoning, the facts are unmistakably in accordance with the conclusion to which the reasoning points. Yet some few still entertain doubts, a circumstance which may prove a source of consolation to any who find themselves unable to follow the reasoning on which the effects of approach and recession on wave-lengths depend. Let such remember, however, that experiment in the case of the aerial waves producing sound, accords perfectly with theory, and that the waves which produce light are perfectly analogous (so far as this particular point is concerned) with the waves producing sound.

Ordinary white light, and many kinds of coloured light, may be compared with *noise*—that is, with a multitude of intermixed sounds. But light of one pure colour may be compared to sound of one determinate note. As the aerial waves producing the effect of one definite tone are all of one length, so the ethereal waves producing light of one definite colour are all of one length. Therefore if we approach or recede from a source of light emitting such waves, effects will result corresponding with what has been described above for the case of water-waves and sound-waves. If we approach the source of light, or if it approaches us, the waves will be shortened; if we recede from it, or if it recedes from us, the waves will be lengthened. But the colour of light depends on its wave-length, precisely as the tone of sound depends on its wave-length. The waves producing red light are longer than those producing orange light, these are longer than the waves producing yellow light; and so the wave-lengths shorten down from yellow to green, thence to blue, to indigo, and finally to violet. Thus if a body shining

in reality with a pure green colour, approached the observer with a velocity comparable with that of light, it would seem blue, indigo, or violet, according to the rate of approach; whereas if it rapidly receded, it would seem yellow, orange, or red, according to the rate of recession.

Unfortunately in one sense, though very fortunately in many much more important respects, the rates of motion among the celestial bodies are *not* comparable with the velocity of light, but are always so much less as to be almost rest by comparison. The velocity of light is about 187,000 miles per second, or, according to the measures of the solar system at present in vogue (which will shortly have to give place to somewhat larger measures, the result of observations made upon the recent transit of Venus), about 185,000 miles per second. The swiftest celestial motion of which we have ever had direct evidence was that of the comet of the year 1843, which, at the time of its nearest approach to the sun, was travelling at the rate of about 350 miles per second. This, compared with the velocity of light, is as the motion of a person taking six steps a minute, each less than half a yard long, to the rush of the swiftest express train. No body within our solar system can travel faster than this, the motion of a body falling upon the sun from an infinite distance being only about 370 miles per second when it reaches his surface. And though swifter motions probably exist among the bodies travelling around more massive suns than ours, yet of such motions we can never become cognizant. All the motions taking place among the stars themselves would appear to be very much less in amount. The most swiftly moving sun seems to travel but at the rate of about 50 or 60 miles per second.

Now let us consider how far a motion of 100 miles per second might be expected to modify the colour of pure green light—selecting green as the middle colour of the spectrum. The waves producing green light are of such a length, that 47,000 of them scarcely equal in length a single inch. Draw on paper an inch and divide it carefully into

ten equal parts, or take such parts from a well-divided rule ; divide one of these tenths into ten equal parts, as nearly as the eye will permit you to judge ; then one of these parts, or about half the thickness of an average pin, would contain 475 of the waves of pure green light. The same length would equal the length of 440 waves of pure yellow light, and of 511 waves of pure blue light. (The green, yellow, and blue, here spoken of, are understood to be of the precise colour of the middle of the green, yellow, and blue parts of the spectrum.) Thus the green waves must be increased in the proportion of 475 to 440 to give yellow light, or reduced in the proportion of 511 to 475 to give blue light. For the first purpose, the velocity of recession must bear to the velocity of light the proportion which 30 bears to 475, or must be equal to rather more than one-sixteenth part of the velocity of light—say 11,600 miles per second. For the second purpose, the velocity of approach must bear to the velocity of light the proportion which 36 bears to 475, or must be nearly equal to one-thirteenth part of the velocity of light—say 14,300 miles per second. But the motions of the stars and other celestial bodies, and also the motions of matter in the sun, and so forth, are very much less than these. Except in the case of one or two comets (and always dismissing from consideration the amazing apparent velocities with which comets' tails *seem* to be formed), we may take 100 miles per second as the extreme limit of velocity with which we have to deal, in considering the application of our theory to the motions of recession and approach of celestial bodies. Thus in the case of recession the greatest possible change of colour in pure green light would be equivalent to the difference between the medium green of the spectrum, and the colour  $\frac{1-11}{16}$ th part of the way from medium green to medium yellow ; and in the case of approach, the change would correspond to the difference between the medium green and the colour  $\frac{1-14}{3}$ rd part of the way from medium green to medium blue. Let any one look at a spectrum of fair

length, or even at a correctly tinted painting of the solar spectrum, and note how utterly unrecognizable to ordinary vision is the difference of tint for even the twentieth part of the distance between medium green and medium yellow on one side or medium blue on the other, and he will recognize how utterly hopeless it would be to attempt to appreciate the change of colour due to the approach or recession of a luminous body shining with pure green light and moving at the tremendous rate of 100 miles per second. It would be hopeless, even though we had the medium green colour and the changed colour, either towards yellow or towards blue, placed side by side for comparison—how much more when the changed colour would have to be compared with the observer's recollection of the medium colour, as seen on some other occasion !

But this is the least important of the difficulties affecting the application of this method by noting change of colour, as Doppler originally proposed. Another difficulty, which seems somehow to have wholly escaped Doppler's attention, renders the colour test altogether unavailable. We do not get *pure* light from any of the celestial bodies except certain gaseous clouds or nebulae. From every sun we get, as from our own sun, all the colours of the rainbow. There may be an excess of some colours and a deficiency of others in any star, so as to give the star a tint, or even a very decided colour. But even a blood-red star, or a deep-blue or violet star, does not shine with pure light, for the spectroscope shows that the star has other colours than those producing the prevailing tint, and it is only the great *excess* of red rays (all kinds of red, too) or of blue rays (of all kinds), and so on, which makes the star appear red, or blue, and so on, to the eye. By far the greater number of stars or suns show all the colours of the rainbow nearly equally distributed, as in the case of our own sun. Now imagine for a moment a white sun, which had been at rest, to begin suddenly to approach us so rapidly (travelling more than 10,000 miles per second) that the red rays became orange, the

orange became yellow, the yellow green, the green blue, the blue indigo, the indigo violet, while the violet waves became too short to affect the sense of sight. Then, *if that were all*, that sun, being deprived of the red part of its light, would shine with a slightly bluish tinge, owing to the relative superabundance of rays from the violet end of the spectrum. We should be able to recognize such a change, yet not nearly so distinctly as if that sun had been shining with a pure green light, and suddenly beginning to approach us at the enormous rate just mentioned, changed in colour to full blue. *Though*, if that sun were all the time approaching us at the enormous rate imagined, we should be quite unable to tell whether its slightly bluish tinge were due to such motion of approach or to some inherent blueness in the light emitted by the star. Similarly, if a white sun suddenly began to recede so rapidly that its violet rays were turned to indigo, the indigo to blue, and so on, the orange rays turning to red, and the red rays disappearing altogether, then, *if that were all*, its light would become slightly reddish, owing to the relative superabundance of light from the red end of the spectrum; and we might distinguish the change, yet not so readily as if a sun shining with pure green light began to recede at the same enormous rate, and so shone with pure yellow light. *Though*, if that sun were all the time receding at that enormous rate, we should be quite unable to tell whether its slightly reddish hue were due to such motion of recession or to some inherent redness in its own lustre. *But in neither case would that be all.* In the former, the red rays would indeed become orange; but the rays beyond the red, which produce no effect upon vision, would be converted into red rays, and fill up the part of the spectrum deserted by the rays originally red. In the latter, the violet rays would indeed become indigo; but the rays beyond the violet, ordinarily producing no visible effect, would be converted into violet rays, and fill up the part of the spectrum deserted by the rays originally violet. Thus, despite the enormous velocity of approach in one case and

of recession in the other, there would be no change whatever in the colour of the sun in either case. All the colours of the rainbow would still be present in the sun's light, and it would therefore still be a white sun.

Doppler's method would thus fail utterly, even though the stars were travelling hither and thither with motions a hundred times greater than the greatest known stellar motions.

This objection to Doppler's theory, as originally proposed, was considered by me in an article on "Coloured Suns" in *Fraser's Magazine* for January, 1868. His theory, indeed, was originally promulgated not as affording a means of measuring stellar motions, but as a way of accounting for the colours of double stars. It was thus presented by Professor Nichol, in a chapter of his "Architecture of the Heavens," on this special subject:—"The rapid motion of light reaches indeed one of those numbers which reason owns, while imagination ceases to comprehend them; but it is also true that the swiftness with which certain individuals of the double stars sweep past their perihelia, or rather their periasters, is amazing; and in this matter of colours, it must be recollect ed that the question solely regards the difference between the velocities of the waves constituent of colours, at those different stellar positions. Still it is a bold—even a magnificent idea; and if it can be reconciled with the permanent colours of the multitude of stars surrounding us—stars which too are moving in great orbits with immense velocities—it may be hailed almost as a positive discovery. It must obtain confirmation, or otherwise, so soon as we can compare with certainty the observed colorific changes of separate systems with the known fluctuations of their orbital motions."

That was written a quarter of a century ago, when spectroscopic analysis, as we now know it, had no existence. Accordingly, while the fatal objection to Doppler's original theory is overlooked on the one hand, the means of applying the principle underlying the theory, in a much more exact

manner than Doppler could have hoped for, is overlooked on the other. Both points are noted in the article above referred to, in the same paragraph. "We may dismiss," I there stated, "the theory started some years ago by the French astronomer, M. Doppler." But, I presently added, "It is quite clear that the effects of a motion rapid enough to produce such a change" (*i.e.* a change of tint in a pure colour) "would shift the position of the whole spectrum—and this change would be readily detected by a reference to the spectral lines." This is true, even to the word "readily." Velocities which would produce an appreciable change of tint would produce "readily" detectible changes in the position of the spectral lines; the velocities actually existing among the star-motions would produce changes in the position of these lines detectible only with extreme difficulty, or perhaps in the majority of instances not detectible at all.

It has been in this way that the spectroscopic method has actually been applied.

It is easy to perceive the essential difference between this way of applying the method and that depending on the attempted recognition of changes of colour. A dark line in the spectrum marks in reality the place of a missing tint. The tints next to it on either side are present, but the tint between them is wanting. They are changed in colour—very slightly, in fact quite inappreciably—by motions of recession or approach, or, in other words, they are shifted in position along the spectrum, towards the red end for recession, towards the violet end for approach; and of course the dark space between is shifted along with them. One may say that the missing tint is changed. For in reality that is precisely what would happen. If the light of a star at rest gave every tint of the spectrum, for instance, except mid-green alone, and that star approached or receded so swiftly that its motion would change pure green light to pure yellow in one case, or pure blue in the other, then the effect on the spectrum of such a star would be to throw the dark line from the middle of the green part of the spectrum to the

middle of the yellow part in one case, or to the middle of the blue part in the other. The dark line would be quite notably shifted in either case. With the actual stellar motions, though all the lines are more or less shifted, the displacement is always exceedingly minute, and it becomes a task of extreme difficulty to recognize, and still more to measure, such displacement.

When I first indicated publicly (January, 1868) the way in which Doppler's principle could alone be applied, two physicists, Huggins in England and Secchi in Italy, were actually endeavouring, with the excellent spectrosopes in their possession, to apply this method. In March, 1868, Secchi gave up the effort as useless, publicly announcing the plan on which he had proceeded and his failure to obtain any results except negative ones. A month later Huggins also publicly announced the plan on which he had been working, but was also able to state that in one case, that of the bright star Sirius, he had succeeded in measuring a motion in the line of sight, having discovered that Sirius was receding from the earth at the rate of 41·4 miles per second. I say *was* receding, because a part of the recession at the time of observation was due to the earth's orbital motion around the sun. I had, at his request, supplied Huggins with the formula for calculating the correction due to this cause, and, applying it, he found that Sirius is receding from the sun at the rate of about  $29\frac{1}{2}$  miles per second, or some 930 millions of miles per annum.

I am not here specially concerned to consider the actual results of the application of this method since the time of Huggins's first success; but the next chapter of the history of the method is one so interesting to myself personally that I feel tempted briefly to refer to details. So soon as I had heard of Huggins's success with Sirius, and that an instrument was being prepared for him wherewith he might hope to extend the method to other stars, I ventured to make a prediction as to the result which he would obtain whensoever he should apply it to five stars of the seven forming the so-

called Plough. I had found reason to feel assured that these five form a system drifting all together amid stellar space. Satisfied for my own part as to the validity of the evidence, I submitted it to Sir J. Herschel, who was struck by its force. The apparent drift of those stars was, of course, a thwart drift; but if they really were drifting in space, then their motions in the line of sight must of necessity be alike. My prediction, then, was that whosoever Huggins applied to those stars the new method he would find them either all receding at the same rate, or all approaching at the same rate, or else that all *alike* failed to give any evidence at all either of recession or approach. I had indicated the five in the first edition of my "Other Worlds"—to wit, the stars of the Plough, omitting the nearest "pointer" to the pole and the star marking the third horse (or the tip of the Great Bear's tail). So soon as Huggins's new telescope and its spectroscopic adjuncts were in working order, he re-examined Sirius, determined the motions of other stars; and at last on one suitable evening he tested the stars of the Plough. He began with the nearest pointer, and found that star swiftly approaching the earth. He turned to the other pointer, and found it rapidly receding from the earth. Being under the impression that my five included both pointers, he concluded that my prediction had utterly failed, and so went on with his observations, altogether unprejudiced in its favour, to say the least. The next star of the seven he found to be receding at the same rate as the second pointer; the next at the same rate, the next, and the next receding still at the same rate, and lastly the seventh receding at a different rate. Here, then, were five stars all receding at a common rate, and of the other two one receding at a different rate, the other swiftly approaching. Turning next to the work containing my prediction, Huggins found that the five stars thus receding at a common rate were the five whose community of motion I had indicated two years before. Thus the first prediction ever made respecting the motions of the so-called fixed stars was not wanting in success. I would venture to

add that the theory of star-drift, on the strength of which the prediction was made, was in effect demonstrated by the result.

The next application of the new method was one of singular interest. I believe it was Mr. Lockyer who first thought of applying the method to measure the rate of solar hurricanes as well as the velocities of the uprush and downrush of vaporous matter in the atmosphere of the sun. Another spectroscopic method had enabled astronomers to watch the rush of glowing matter from the edge of the sun, by observing the coloured flames and their motions ; but by the new method it was possible to determine whether the flames at the edge were swept by solar cyclones carrying them from or towards the eye of the terrestrial observer, and also to determine whether glowing vapours over the middle of the visible disc were subject to motion of uprush, which of course would carry them towards the eye, or of downrush, which would carry them from the eye. The result of observations directed to this end was to show that at least during the time when the sun is most spotted, solar hurricanes of tremendous violence take place, while the uppushing and downrushing motions of solar matter sometimes attain a velocity of more than 100 miles per second.

It was this success on the part of an English spectroscopist which caused that attack on the new method against which it has but recently been successfully defended, at least in the eyes of those who are satisfied only by experimental tests of the validity of a process. The Padre Secchi had failed, as we have seen, to recognize motions of recession and approach among the stars by the new method. But he had taken solar observation by spectroscopic methods under his special charge, and therefore when the new results reached his ears he felt bound to confirm or invalidate them. He believed that the apparent displacement of dark lines in the solar spectrum might be due to the heat of the sun causing changes in the delicate

adjustments of the instrument—a cause of error against which precautions are certainly very necessary. He satisfied himself that when sufficient precautions are taken no displacements take place such as Lockyer, Young, and others claimed to have seen. But he submitted the matter to a further test. As the sun is spinning swiftly on his axis, his mighty equator, more than two and a half millions of miles in girth, circling once round in about twenty-four days, it is clear that on one side the sun's surface is swiftly moving *towards*, and on the other side as swiftly moving *from*, the observer. By some amazing miscalculation, Secchi made the rate of this motion 20 miles per second, so that the sum of the two motions in opposite directions would equal 40 miles per second. He considered that he ought to be able by the new method, if the new method is trustworthy at all, to recognize this marked difference between the state of the sun's eastern and western edges ; he found on trial that he could not do so ; and accordingly he expressed his opinion that the new method is not trustworthy, and that the arguments urged in its favour are invalid.

The weak point in his reasoning resided in the circumstance that the solar equator is only moving at the rate of about  $1\frac{1}{4}$  miles per second, so that instead of a difference of 40 miles per second between the two edges, which should be appreciable, the actual difference (that is, the sum of the two equal motions in opposite directions) amounts only to  $2\frac{1}{2}$  miles per second, which certainly Secchi could not hope to recognize with the spectroscopic power at his disposal. Nevertheless, when the error in his reasoning was pointed out, though he admitted that error, he maintained the justice of his conclusion ; just as Cassini, having mistakenly reasoned that the degrees of latitude should diminish towards the pole instead of increasing, and having next mistakenly found, as he supposed, that they do diminish, acknowledged the error of his reasoning, but insisted on the validity of his observations,—maintaining

thenceforth, as all the world knows, that the earth is extended instead of flattened at the poles.

Huggins tried to recognize by the new method the effects of the sun's rotation, using a much more powerful spectroscope than Sècchi's. The history of the particular spectroscope he employed is in one respect specially interesting to myself, as the extension of spectroscopic power was of my own devising before I had ever used or even seen a powerful spectroscope. The reader is aware that spectrosopes derive their light-sifting power from the prisms forming them. The number of prisms was gradually increased, from Newton's single prism to Fraunhofer's pair, and to Kirchhoff's battery of four, till six were used, which bent the light round as far as it would go. Then the idea occurred of carrying the light to a higher level (by reflections) and sending it back through the same battery of prisms, doubling the dispersion. Such a battery, if of six prisms, would spread the spectral colours twice as widely apart as six used in the ordinary way, and would thus have a dispersive power of twelve prisms. It occurred to me that after taking the rays through six prisms, arranged in a curve like the letter C, an intermediate four-cornered prism of a particular shape (which I determined) might be made to send the rays into another battery of six prisms, the entire set forming a double curve like the letter S, the rays being then carried to a higher level and back through the double battery. In this way a dispersive power of nineteen prisms could be secured. My friend, Mr. Browning, the eminent optician, made a double battery of this kind,\*

\* I have omitted all reference to details; but in reality the double battery was automatic, the motion of the observing telescope, as different colours of the spectrum were brought into view, setting all the prisms of the double battery into that precise position which causes them to show best each particular part of the spectrum thus brought into view. It is rather singular that the first view I ever had of the solar prominences, was obtained (at Dr. Huggins's observatory) with this instrument of my own invention, which also was the first powerful spectroscope I had ever used or even seen.

which was purchased by Mr. W. Spottiswoode, and by him lent to Mr. Huggins for the express purpose of dealing with the task Secchi had set spectroscopists. It did not, however, afford the required evidence. Huggins considered the displacement of dark lines due to the sun's rotation to be recognizable, but so barely that he could not speak confidently on the point.

There for a while the matter rested. Vögel made observations confirming Huggins's results relative to stellar motions; but Vögel's instrumental means were not sufficiently powerful to render his results of much weight.

But recently two well-directed attacks have been made upon this problem, one in England, the other in America, and in both cases with success. Rather, perhaps, seeing that the method had been attacked and was supposed to require defence, we may say that two well-directed assaults have been made upon the attacking party, which has been completely routed.

Arrangements were made not very long ago, by which the astronomical work of Greenwich Observatory, for a long time directed almost exclusively to time observations, should include the study of the sun, stars, planets, and so forth. Amongst other work which was considered suited to the National Observatory was the application of spectroscopic analysis to determine motions of recession and approach among the celestial bodies. Some of these observations, by the way, were made, we are told, "to test the truth of Doppler's principle," though it seems difficult to suppose for an instant that mathematicians so skilful as the chief of the Observatory and some of his assistants could entertain any doubt on that point. Probably it was intended by the words just quoted to imply simply that some of the observations were made for the purpose of illustrating the principle of the method. We are not to suppose that on a point so simple the Greenwich observers have been in any sort of doubt.

At first their results were not very satisfactory. The

difficulties which had for a long time foiled Huggins, and which Secchi was never able to master, rendered the first Greenwich measures of stellar motions in the line of sight wildly inconsistent, not only with Huggins's results, but with each other.

Secchi was not slow to note this. He renewed his objections to the new method of observation, pointing and illustrating them by referring to the discrepancies among the Greenwich results. But recently a fresh series of results has been published, showing that the observers at Greenwich have succeeded in mastering some at least among the difficulties which they had before experienced. The measurements of star-motions showed now a satisfactory agreement with Huggins's results, and their range of divergence among themselves was greatly reduced. The chief interest of the new results, however, lay in the observations made upon bodies known to be in motion in the line of sight at rates already measured. These observations, though not wanted as tests of the accuracy of the principle, were very necessary as tests of the qualities of the instruments used in applying it. It is here and thus that Secchi's objections alone required to be met, and here and thus they have been thoroughly disposed of. Let us consider what means exist within the solar system for thus testing the new method.

The earth travels along in her orbit at the rate of about  $18\frac{1}{3}$  miles in every second of time. Not to enter into niceties which could only properly be dealt with mathematically, it may be said that with this full velocity she is at times approaching the remoter planets of the system, and at times receding from them; so that here at once is a range of difference amounting to about 37 miles per second, and fairly within the power of the new method of observation. For it matters nothing, so far as the new method is concerned, whether the earth is approaching another orb by her motion, or that orb approaching by its own motion. Again, the planet Venus travels at the rate of about  $21\frac{1}{2}$  miles per second, but as the earth travels only 3 miles a second less

swiftly, and the same way round, only a small portion of Venus's motion ever appears as a motion of approach towards or recession from the earth. Still, Venus is sometimes approaching and sometimes receding from the earth, at a rate of more than 8 miles per second. Her light is much brighter than that of Jupiter or Saturn, and accordingly this smaller rate of motion would be probably more easily recognized than the greater rate at which the giant planets are sometimes approaching and at other times receding from the earth. At least, the Greenwich observers seem to have confined their attention to Venus, so far as motions of planets in the line of sight are concerned. The moon, as a body which keeps always at nearly the same distance from us, would of course be the last in the world to be selected to give positive evidence in favour of the new method; but she serves to afford a useful test of the qualities of the instruments employed. If when these were applied to her they gave evidence of motions of recession or approach at the rate of several miles per second, when we know as a matter of fact that the moon's distance never\* varies by more than 30,000 miles during the lunar month, her rate of approach or recession thus averaging about one-fiftieth part of a mile per second, discredit would be thrown on the new method—not, indeed, as regards its principle, which no competent reasoner can for a moment question, but as regards the possibility of practically applying it with our present instrumental means.

Observations have been made at Greenwich, both on Venus and on the moon, by the new method, with results entirely satisfactory. The method shows that Venus is receding when she is known to be receding, and that she is approaching when she is known to be approaching. Again, the method shows no signs of approach or recession in the moon's case. It is thus in satisfactory agreement with the

\* It varies more in some months than in others, as the moon's orbit changes in shape under the various perturbing influences to which she is subject.

known facts. Of course these results are open to the objection that the observers have known beforehand what to expect, and that expectation often deceives the mind; especially in cases where the thing to be observed is not at all easy to recognize. It will presently be seen that the new method has been more satisfactorily tested, in this respect, in other ways. It may be partly due to the effect of expectation that in the case of Venus the motions of approach and recession, tested by the new method, have always been somewhat too great. A part of the excess may be due to the use of the measure of the sun's distance, and therefore the measures of the dimensions of the solar system, in vogue before the recent transit. These measures fall short to some degree of those which result from the observations made in December, 1874, on Venus in transit, the sun's distance being estimated at about 91,400,000 miles instead of 92,000,000 miles, which would seem to be nearer the real distance. Of course all the motions within the solar system would be correspondingly under-estimated. On the other hand, the new method would give all velocities with absolute correctness if instrumental difficulties could be overcome. The difference between the real velocities of Venus approaching and receding, and those calculated according to the present inexact estimate of the sun's distance, is however much less than the observed discrepancy, doubtless due to the difficulties involved in the application of this most difficult method. I note the point, chiefly for the sake of mentioning the circumstance that theoretically the method affords a new means of measuring the dimensions of the solar system. Whencever the practical application of the method has been so far improved that the rate of approach or recession of Venus, or Mercury, or Jupiter, or Saturn (any one of these planets), can be determined on any occasion, with great nicety, we can at once infer the sun's distance with corresponding exactness. Considering that the method has only been invented ten years (setting aside Doppler's first vague ideas respecting it), and that spectroscopic analysis as a method of exact

observation is as yet little more than a quarter of a century old, we may fairly hope that in the years to come the new method, already successfully applied to measure motions of recession and approach at the rate of 20 or 30 miles per second, will be employed successfully in measuring much smaller velocities. Then will it give us a new method of measuring the great base-line of astronomical surveying—the distance of our world from the centre of the solar system.

That this will one day happen is rendered highly probable, in my opinion, by the successes next to be related.

Besides the motions of the planets around the sun, there are their motions of rotation, and the rotation of the sun himself upon his axis. Some among these turning motions are sufficiently rapid to be dealt with by the new method. The most rapid rotational motion with which we are acquainted from actual observation is that of the planet Jupiter. The circuit of his equator amounts to about 267,000 miles, and he turns once on his axis in a few minutes less than ten hours, so that his equatorial surface travels at the rate of about 26,700 miles an hour, or nearly  $7\frac{1}{2}$  miles per second. Thus between the advancing and retreating sides of the equator there is a difference of motion in the line of sight amounting to nearly 15 miles. But this is not all. Jupiter shines by reflecting sunlight. Now it is easily seen that where his turning equator *meets* the waves of light from the sun, these are shortened, in the same sense that waves are shortened for a swimmer travelling to meet them, while these waves, already shortened in this way, are further shortened when starting from the same advancing surface of Jupiter, on their journey to us after reflection. In this way the shortening of the waves is doubled, at least when the earth is so placed that Jupiter lies in the same direction from us as from the sun, the very time, in fact, when Jupiter is most favourably placed for ordinary observation, or is at his highest due south, when the sun is at his lowest below the northern horizon—that is, at midnight. The lengthening

of the waves is similarly doubled at this most favourable time for observation ; and the actual difference between the motion of the two sides of Jupiter's equator being nearly 15 miles per second, the effect on the light-waves is equivalent to that due to a difference of nearly 30 miles per second. Thus the new method may fairly be expected to indicate Jupiter's motion of rotation. The Greenwich observers have succeeded in applying it, though Jupiter has not been favourably situated for observation. Only on one occasion, says Sir G. Airy, was the spectrum of Jupiter "seen fairly well," and on that occasion "measures were obtained which gave a result in remarkable agreement with the calculated value." It may well be hoped that when in the course of a few years Jupiter returns to that part of his course where he rises high above the horizon, shining more brightly and through a less perturbed air, the new method will be still more successfully applied. We may even hope to see it extended to Saturn, not merely to confirm the measures already made of Saturn's rotation, but to resolve the doubts which exist as to the rotation of Saturn's ring-system.

Lastly, there remains the rotation of the sun, a movement much more difficult to detect by the new method, because the actual rate of motion even at the sun's equator amounts only to about 1 mile per second.

In dealing with this very difficult task, the hardest which spectroscopists have yet attempted, the Greenwich observers have achieved an undoubted success ; but unfortunately for them, though fortunately for science, another observatory, far smaller and of much less celebrity, has at the critical moment achieved success still more complete.

The astronomers at our National Observatory have been able to recognize by the new method the turning motion of the sun upon his axis. And here we have not, as in the case of Venus, to record merely that the observers have seen what they expected to see because of the known motion of the sun. "Particular care was taken," says

Airy, "to avoid any bias from previous knowledge of the direction in which a displacement" (of the spectral lines) "was to be expected," the side of the sun under observation not being known by the observer until after the observation was completed.

But Professor Young, at Dartmouth College, Hanover, N.H., has done much more than merely obtain evidence by the new method that the sun is rotating as we already knew. He has succeeded so perfectly in mastering the instrumental and observational difficulties, as absolutely to be able to rely on his *measurement* (as distinguished from the mere recognition) of the sun's motion of rotation. The manner in which he has extended the powers of ordinary spectroscopic analysis, cannot very readily be described in these pages, simply because the principles on which the extension depends require for their complete description a reference to mathematical considerations of some complexity. Let it be simply noted that what is called the diffraction spectrum, obtained by using a finely lined plate, results from the dispersive action of such a plate, or *grating* as it is technically called, and this dispersive power can be readily combined with that of a spectroscope of the ordinary kind. Now Dr. Rutherford, of New York, has succeeded in ruling so many thousand lines on glass within the breadth of a single inch as to produce a grating of high dispersive power. Availing himself of this beautiful extension of spectroscopic powers, Professor Young has succeeded in recognizing effects of much smaller motions of recession and approach than had before been observable by the new method. He has thus been able to measure the rotation-rate of the sun's equatorial regions. His result exceeds considerably that inferred from the telescopic observation of the solar spots. For whereas from the motion of the spots a rotation-rate of about  $1\frac{1}{4}$  mile per second has been calculated for the sun's equator, Professor Young obtains from his spectroscopic observations a rate of rather more than  $1\frac{2}{5}$  mile, or about 300 yards per second more than the telescopic rate.

If Young had been measuring the motion of the same matter which is observed with the telescope, there could of course be no doubt that the telescope was right and the spectroscopic wrong. We might add a few yards per second for the probably greater distance of the sun resulting from recent transit observations. For of course with an increase in our estimate of the sun's distance there comes an increase in our estimate of the sun's dimensions, and of the velocity of the rotational motion of his surface. But only about 12 yards per second could be allowed on this account; the rest would have to be regarded as an error due to the difficulties involved in the spectroscopic method. In reality, however, the telescopist and the spectroscopist observe different things in determining by their respective methods the sun's motion of rotation. The former observes the motion of the spots belonging to the sun's visible surface; the latter observes the motion of the glowing vapours outside that surface, for it is from these vapours, not from the surface of the sun, that the dark lines of the spectrum proceed. Now so confident is Professor Young of the accuracy of his spectroscopic observations, that he is prepared to regard the seeming difference of velocity between the atmosphere and surface of the sun as real. He believes that "the solar atmosphere really sweeps forward over the underlying surface, in the same way that the equatorial regions outstrip the other parts of the sun's surface." This inference, important and interesting in itself, is far more important in what it involves. For if we can accept it, it follows that the spectroscopic method of measuring the velocity of motions in the line of sight is competent, under favourable conditions, to obtain results accurate within a few hundred yards per second, or 10 or 12 miles per minute. If this shall really prove to be true for the method now, less than ten years after it was first successfully applied, what may we not hope from the method in future years? Spectroscopic analysis itself is in its infancy, and this method is but a recent application

of spectroscopy. A century or so hence astronomers will smile (though not disdainfully) at these feeble efforts, much as we smile now in contemplating the puny telescopes with which Galileo and his contemporaries studied the star-depths. And we may well believe that largely as the knowledge gained by telescopists in our own time surpasses that which Galileo obtained, so will spectroscopists a few generations hence have gained a far wider and deeper insight into the constitution and movements of the stellar universe than the spectroscopists of our own day dare even hope to attain.

I venture confidently to predict that, in that day, astronomers will recognize in the universe of stars a variety of structure, a complexity of arrangement, an abundance of every form of cosmical vitality, such as I have been led by other considerations to suggest, not the mere cloven lamina of uniformly scattered stars more or less resembling our sun, and all in nearly the same stage of cosmical development, which the books of astronomy not many years since agreed in describing. The history of astronomical progress does not render it probable that the reasoning already advanced, though in reality demonstrative, will convince the generality of science students until direct and easily understood observations have shown the real nature of the constitution of that part of the universe over which astronomical survey extends. But the evidence already obtained, though its thorough analysis may be "*caviare* to the general," suffices to show the real nature of the relations which one day will come within the direct scope of astronomical observation.

## *THE NEW STAR WHICH FADED INTO STAR-MIST.*

THE appearance of a new star in the constellation of the Swan in the autumn of 1876 promises to throw even more light than was expected on some of the most interesting problems with which modern astronomy has to deal. It was justly regarded as a circumstance of extreme interest that so soon after the outburst of the star which formed a new gem in the Northern Crown in May, 1866, another should have shone forth under seemingly similar conditions. And when, as time went on, it appeared that in several respects the new star in the Swan differed from the new star in the Crown, astronomers found fresh interest in studying, as closely as possible, the changes presented by the former as it gradually faded from view. But they were not prepared to expect what has actually taken place, or to recognize so great a difference of character between these two new stars, that whereas one seemed throughout its visibility to ordinary eyesight, and even until the present time, to be justly called a star, the other should so change as to render it extremely doubtful whether at any time it deserved to be regarded as a star or sun.

Few astronomical phenomena, even of those observed during this century (so fruitful in great astronomical discoveries), seem better worthy of thorough investigation and study than those presented by the two stars which appeared in the Crown and in the Swan, in 1866 and 1876 respec-

tively. A new era seems indeed to be beginning for those departments of astronomy which deal with stars and star-cloudlets on the one hand, and with the evolution of solar systems and stellar systems on the other.

Let us briefly consider the history of the star of 1866 in the first place, and then turn our thoughts to the more surprising and probably more instructive history of the star which shone out in November, 1876.

In the first place, however, I would desire to make a few remarks on the objections which have been expressed by an observer to whom astronomy is indebted for very useful work, against the endeavour to interpret the facts ascertained respecting these so-called new stars. M. Cornu, who made some among the earliest spectroscopic observations of the star in Cygnus, after describing his results, proceeded as follows :—"Grand and seductive though the task may be of endeavouring to draw from observed facts inductions respecting the physical state of this new star, respecting its temperature, and the chemical reactions of which it may be the scene, I shall abstain from all commentary and all hypothesis on this subject. I think that we do not yet possess the data necessary for arriving at useful conclusions, or at least at conclusions capable of being tested: however attractive hypotheses may be, we must not forget that they are outside the bounds of science, and that, far from serving science, they seriously endanger its progress." This, as I ventured to point out at the time, is utterly inconsistent with all experience. M. Cornu's objection to theorizing when he did not see his way to theorizing justly, is sound enough; but his general objection to theorizing is, with all deference be it said, sheerly absurd. It will be noticed that I say theorizing, not hypothesis-framing; for though he speaks of hypotheses, he in reality is describing theories. The word hypothesis is too frequently used in this incorrect sense—perhaps so frequently that we may almost prefer sanctioning the use to substituting the correct word. But the fact really is, that many, even among scientific writers,

when they hear the word hypothesis, think immediately of Newton's famous "hypotheses non fingo," a dictum relating to real hypotheses, not<sup>1</sup> to theories. It would, in fact, be absurd to suppose that Newton, who had advanced, advocated, and eventually established, the noblest scientific theory the world has known, would ever have expressed an objection to theorizing, as he is commonly understood to have done by those who interpret his "hypotheses non fingo" in the sense which finds favour with M. Cornu. But apart from this, Newton definitely indicates what he means by hypotheses. "I frame no hypotheses," he says, "*for whatever is not deduced from phenomena is to be called an hypothesis.*" M. Cornu, it will be seen, rejects the idea of deducing from phenomena what he calls an hypothesis, but what would not be an hypothesis according to Newton's definition: "Malgré tout ce qu'il y aurait de séduisant et de grandiose à tirer de ce fait des inductions, etc., je m'abstiens-drai de tout commentaire et de toute hypothèse à ce sujet." It is not thus that observed scientific facts are to be made fruitful, nor thus that the points to which closer attention must be given are to be ascertained.

Since the preceding paragraph was written, my attention has been attracted to the words of another observer more experienced than M. Cornu, who has not only expressed the same opinion which I entertain respecting M. Cornu's ill-advised remark, but has illustrated in a very practical way, and in this very case, how science gains from commentary and theory upon observed facts. Herr Vögel considers "that the fear that an hypothesis" (he, also, means a theory here) "might do harm to science is only justifiable in very rare cases : in most cases it will further science. In the first place, it draws the attention of the observer to things which but for the hypothesis might have been neglected. Of course if the observer is so strongly influenced that in favour of an hypothesis he sees things which do not exist—and this may happen sometimes—science may for a while be arrested in its progress, but in that case the observer is

far more to blame than the author of the hypothesis. On the other hand, it is very possible that an observer may, involuntarily, arrest the progress of science, even without originating an hypothesis, by pronouncing and publishing sentences which have a tendency to diminish the general interest in a question, and which do not place its high significance in the proper light." (This is very neatly put.) He is "almost inclined to think that such an effect might follow from the reading of M. Cornu's remark, and that nowhere better than in the present case, where in short periods colossal changes showed themselves occurring upon a heavenly body, might the necessary data be obtained for drawing useful conclusions, and tests be applied to those hypotheses which have been ventured with regard to the condition of heavenly bodies." It was, as we shall presently see, in thus collecting data and applying tests, that Vögel practically illustrated the justice of his views.

The star which shone out in the Northern Crown in May, 1866, would seem to have grown to its full brightness very quickly. It is not necessary that I should here consider the history of the star's discovery; but I think all who have examined that history agree in considering that whereas on the evening of May 12, 1866, a new star was shining in the Northern Crown with second-magnitude brightness, none had been visible in the same spot with brightness above that of a fifth-magnitude star twenty-four hours earlier. On ascertaining, however, the place of the new star, astronomers found that there had been recorded in Argelander's charts and catalogue a star of between the ninth and tenth magnitude in this spot. The star declined very rapidly in brightness. On May 13th it appeared of the third magnitude; on May 16th it had sunk to the fourth magnitude; on the 17th to the fifth; on the 19th to the seventh; and by the end of the month it shone only as a telescopic star of the ninth magnitude. It is now certainly not above the tenth magnitude.

Examined with the spectroscope, this star was found to

be in an abnormal condition. It gave the rainbow-tinted streak crossed by dark lines, which is usually given by stars (with minor variations, which enable astronomers to classify the stars into several distinct orders). But superposed upon this spectrum, or perhaps we should rather say shining through this spectrum, were seen four brilliant lines, two of which certainly belonged to glowing hydrogen. These lines were so bright as to show that the greater part of the light of the star at the time came from the glowing gas or gases giving these lines. It appeared, however, that the rainbow-tinted spectrum on which these lines were seen was considerably brighter than it would otherwise have been, in consequence of the accession of heat indicated by and probably derived from the glowing hydrogen.

Unfortunately, we have not accordant accounts of the changes which the spectrum of this star underwent as the star faded out of view. Wolf and Rayet, of the Paris Observatory, assert that when there remained scarcely any trace of the continuous spectrum, the four bright lines were still quite brilliant. But Huggins affirms that this was not the case in his observations; he was "able to see the continuous spectrum when the bright lines could be scarcely distinguished." As the bright lines certainly faded out of view eventually, we may reasonably assume that the French observers were prevented by the brightness of the lines from recognizing the continuous spectrum at that particular stage of the diminution of the star's light when the continuous spectrum had faded considerably but the hydrogen lines little. Later, the continuous spectrum ceased to diminish in brightness, while the hydrogen lines rapidly faded. Thereafter the continuous spectrum could be discerned, and with greater and greater distinctness as the hydrogen lines faded out.

Now, in considering the meaning of the observed changes in the so-called "new star," we have two general theories to consider.

One of these theories is that to which Dr. Huggins would seem to have inclined, though he did not definitely

adopt it—the theory, namely, that in consequence of some internal convulsion enormous quantities of hydrogen and other gases were evolved, which in combining with some other elements ignited on the surface of the star, and thus enveloped the whole body suddenly in a sheet of flame.

"The ignited hydrogen gas in burning produced the light corresponding to the two bright bands in the red and green; the remaining bright lines were not, however, coincident with those of oxygen, as might have been expected. According to this theory, the burning hydrogen must have greatly increased the heat of the solid matter of the photosphere and brought it into a state of more intense incandescence and luminosity, which may explain how the formerly faint star could so suddenly assume such remarkable brilliancy; the liberated hydrogen became exhausted, the flame gradually abated, and with the consequent cooling the photosphere became less vivid, and the star returned to its original condition."

According to the other theory, advanced by Meyer and Klein, the blazing forth of this new star may have been occasioned by the violent precipitation of some great mass, perhaps a planet, upon a fixed star, "by which the momentum of the falling mass would be changed into molecular motion," and result in the emission of light and heat.

"It might even be supposed that the new star, through its rapid motion, may have come in contact with one of the nebulæ which traverse in great numbers the realms of space in every direction, and which from their gaseous condition must possess a high temperature; such a collision would necessarily set the star in a blaze, and occasion the most vehement ignition of its hydrogen."

If we regard these two theories in their more general aspect, considering one as the theory that the origin of disturbance was within the star, and the other as the theory that the origin of disturbance was outside the star, they seem to include all possible interpretations of the observed

phenomena. But, as actually advanced, neither seems satisfactory. The sudden pouring forth of hydrogen from the interior, in quantities sufficient to explain the outburst, seems altogether improbable. On the other hand, as I have pointed out elsewhere, there are reasons for rejecting the theory that the cause of the heat which suddenly affected this star was either the downfall of a planet on the star or the collision of the star with a star-cloudlet or nebula, traversing space in one direction, while the star rushed onwards in another.

A planet could not very well come into final conflict with its sun at one fell swoop. It would gradually draw nearer and nearer, not by the narrowing of its path, but by the change of the path's shape. The path would, in fact, become more and more eccentric; until at length, at its point of nearest approach, the planet would graze its primary, exciting an intense heat where it struck, but escaping actual destruction that time. The planet would make another circuit, and again graze the sun, at or near the same part of the planet's path. For several circuits this would continue, the grazes not becoming more and more effective each time, but rather less. The interval between them, however, would grow continually less and less; at last the time would come when the planet's path would be reduced to the circular form, its globe touching the sun's all the way round, and then the planet would very quickly be reduced to vapour and partly burned up, its substance being absorbed by its sun. But all successive grazes would be indicated to us by accessions of lustre, the period between each seeming outburst being only a few months at first, and gradually becoming less and less (during a long course of years, perhaps even of centuries) until the planet was finally destroyed. Nothing of this sort has happened in the case of any so-called new star. As for the rush of a star through a nebulous mass," that is a theory which would scarcely be entertained by any one acquainted with the enormous distances separating the gaseous star-clouds

properly called nebulæ. There may be small clouds of the same sort scattered much more freely through space ; but we have not a particle of evidence that this is actually the case. All we certainly *know* about star-cloudlets suggests that the distances separating them from each other are comparable with those which separate star from star, in which case the idea of a star coming into collision with a star-cloudlet, and still more the idea of this occurring several times in a century, is wild in the extreme.

But while thus advancing objections, which seem to me irrefragable, against the theory that either a planet or a nebula (still less another small star) had come into collision with the orb in Corona which shone out so splendidly for a while, I advanced another view which seemed to me then and seems now to correspond well with phenomena, and to render the theory of action from without on the whole preferable to the theory of outburst from within. I suggested that, far more probably, an enormous flight of large meteoric masses travelling around the star had come into partial collision with it in the same way that the flight of November meteors comes into collision with our earth thrice in each century, and that other meteoric flights may occasionally come into collision with our sun, producing the disturbances which occasion the sun-spots. As I pointed out, in conceiving this we are imagining nothing new. A meteoric flight capable of producing the suggested effects would differ only in kind from meteoric flights which are known to circle around our own sun. The meteors which produce the November displays of falling stars follow in the track of a comet barely visible to the naked eye.

"May we not reasonably assume that those glorious comets which have not only been visible but conspicuous, shining even in the day-time, and brandishing around tails, which like that of the 'wonder in heaven, the great dragon,' seemed to 'draw the third part of the stars of heaven,' are followed by much denser flights of much more massive meteors? Some of these giant comets have paths which

carry them very close to our sun. Newton's comet, with its tail a hundred millions of miles in length, all but grazed the sun's globe. The comet of 1843, whose tail, says Sir John Herschel, 'stretched half-way across the sky,' must actually have grazed the sun, though but lightly, for its nucleus was within 80,000 miles of his surface, and its head was more than 160,000 miles in diameter. And these are only two among the few comets whose paths are known. At any time we might be visited by a comet mightier than either, travelling in an orbit intersecting the sun's surface, followed by flights of meteoric masses enormous in size and many in number, which, falling on the sun's globe with enormous velocity corresponding to their vast orbital range and their near approach to the sun—a velocity of some 360 miles per second—would, beyond all doubt, excite his whole frame, and especially his surface regions, to a degree of heat far exceeding what he now emits."

This theory corresponds far better also with observed facts than the theory of Meyer and Klein, in other respects than simply in antecedent probability. It can easily be shown that if a planet fell upon a sun in such sort as to become part of his mass, or if a nebula in a state of intense heat excited the whole frame of a star to a similar degree of heat, the effects would be of longer duration than the observed accession of heat and light in the case of all the so-called "new stars." It has been calculated by Mr. Croll (the well-known mathematician to whom we owe the most complete investigations yet made into the effect of the varying eccentricity of the earth's orbit on the climate of the earth) that if two suns, each equal in mass to one-half of our sun, came into collision with a velocity of 476 miles per second, light and heat would be produced which would cover the present rate of the sun's radiation for fifty million years. Now although it certainly does not follow from this that such a collision would result in the steady emission of so much light and heat as our sun gives out, for a period of fifty million years, but is, on the contrary, certain that there

would be a far greater emission at first and a far smaller emission afterwards, yet it manifestly must be admitted that such a collision could not possibly produce so short-lived an effect as we see in the case of every one of the so-called new stars. The diminution in the emission of light and heat from the maximum to one-half the maximum would not occupy fifty millions of years, or perhaps even five million or five hundred thousand years; but it would certainly require thousands of years; whereas we have seen that the new stars in the Crown and in the Swan have lost not one-half but ninety-nine hundredths of their maximum lustre in a few months.

This has been urged as an objection even to the term star as applied to these suddenly appearing orbs. But the objection is not valid; because there is no reason whatever for supposing that even our own sun might not be excited by the downfall of meteoric or cometic matter upon it to a sudden and short-lasting intensity of splendour and of heat. Mr. Lockyer remarks that, if any star, properly so called, were to become a “*a world on fire*,” or “*burst into flames*,” or, in less poetical language, were to be driven either into a condition of incandescence absolutely, or to have its incandescence increased, there can be little doubt that thousands or millions of years would be necessary for the reduction of its light to its original intensity. This must, however, have been written in forgetfulness of some facts which have been ascertained respecting our sun, and which indicate pretty clearly that the sun’s surface might be roused to a temporary intensity of splendour and heat without any corresponding increase in the internal heat, or in the activity of the causes, whatever they may be, to which the sun’s *steady* emissions of light and heat are due.

For instance, most of my readers are doubtless familiar with the account (an oft-told tale, at any rate) of the sudden increase in the splendour of a small portion of the sun’s surface on September 1, 1859, observed by two astronomers independently. The appearances described corresponded

exactly with what we should expect if two large meteoric masses travelling side by side had rushed, with a velocity originally amounting to two or three hundred miles per second, through the portions of the solar atmosphere lying just above, at, and just below the visible photosphere. The actual rate of motion was measured at 120 miles per second as the minimum, but may, if the direction of motion was considerably inclined to the line of sight, have amounted to more than 200 miles per second. The effect was such, that the parts of the sun thus suddenly excited to an increased emission of light and heat appeared like bright stars upon the background of the glowing photosphere itself. One of the observers, Carrington, supposed for a moment that the dark glass screen used to protect the eye had broken. The increase of splendour was exceedingly limited in area, and lasted only for a few minutes—fortunately for the inhabitants of earth. As it was, the whole frame of the earth sympathized with the sun. Vivid auroras were seen, not only in both hemispheres, but in latitudes where auroras are seldom seen. They were accompanied by unusually great electro-magnetic disturbances.

"In many places," says Sir J. Herschel, "the telegraph wires struck work. At Washington and Philadelphia, the electric signalmen received severe electric shocks. At a station in Norway, the telegraphic apparatus was set fire to, and at Boston, in North America, a flame of fire followed the pen of Bain's electric telegraph, which writes down the message upon chemically prepared paper."

We see, then, that most certainly the sun can be locally excited to increased emission of light and heat, which nevertheless may last but for a very short time; and we have good reason for believing that the actual cause of the sudden change in his condition was the downfall of meteoric matter upon a portion of his surface. We may well believe that, whatever the cause may have been, it was one which might in the case of other suns, or even in our sun's own case, affect a much larger portion of the photosphere. If this hap-

pened there would be just such an accession of splendour as we recognize in the case of the new stars. And as the small local accession of brilliancy lasted only a few minutes, we can well believe that an increase of surface brilliancy affecting a much larger portion of the photosphere, or even the entire photosphere, might last but for a few days or weeks.

All that can be said in the way of negative evidence, so far as our own sun is concerned, is that we have no reason for believing that our sun has, at any time within many thousands of years, been excited to emit even for a few hours a much greater amount of light and heat than usual ; so that it has afforded no direct evidence in favour of the belief that other suns may be roused to many times their normal splendour, and yet very quickly resume that usual lustre. But we know that our sun, whether because of his situation in space, or of his position in time (that is, the stage of solar development to which he has at present attained), belongs to the class of stars which shine with steady lustre. He does not vary like Betelgeux, for example, which is not only a sun like him as to general character, but notably a larger and more massive orb. Still less is he like Mira, the Wonderful Star ; or like that more wonderful variable star, Eta Argûs, which at one time shines with a lustre nearly equalling that of the bright Sirius, and anon fades away almost into utter invisibility. He *is* a variable sun, for we cannot suppose that the waxing and waning of the sun-spot period leaves his lustre, as a whole, altogether unaffected. But his variation is so slight that, with all ordinary methods of photometric measurement by observers stationed on worlds which circle around other suns, it must be absolutely undiscernible. We do not, however, reject Betelgeux, or Mira, or even Eta Argûs, from among stars because they vary in lustre. We recognize the fact that, as in glory, so in condition and in changes of condition, one star differeth from another.

Doubtless there are excellent reasons for rejecting the theory that a massive body like a planet, or a nebulous mass like those which are found among the star-depths (the least

of which would exceed many times in volume a sphere filling the entire space of the orbit of Neptune), fell on some remote sun in the Northern Crown. But there are no sufficient reasons for rejecting or even doubting the theory that a comet, bearing in its train a flight of many millions of meteoric masses, falling directly upon such a sun, might cause it to shine with many times its ordinary lustre, but only for a short time, a few months or weeks, or a few days, or even hours. In the article entitled "Suns in Flames," in my "Myths and Marvels of Astronomy," before the startling evidence recently obtained from the star in Cygnus had been thought of, I thus indicated the probable effects of such an event :—"When the earth has passed through the richer portions (not the actual nuclei be it remembered) of meteor systems, the meteors visible from even a single station have been counted by tens of thousands, and it has been computed that millions must have fallen upon the whole earth. These were meteors following in the trains of very small comets. If a very large comet followed by no denser a flight of meteors, but each meteoric mass much larger, fell directly upon the sun, it would not be the outskirts but the nucleus of the meteoric train which would impinge upon him. They would number thousands of millions. The velocity of downfall of each mass would be more than 360 miles per second. And they would continue to pour in upon him for several days in succession, millions falling every hour. It seems not improbable that under this tremendous and long-continued meteoric hail, his whole surface would be caused to glow as intensely as that small part whose brilliancy was so surprising in the observation made by Carrington and Hodgson. In that case our sun, seen from some remote star whence ordinarily he is invisible, would shine out as a new sun for a few days, while all things living, on our earth and whatever other members of the solar system are the abodes of life, would inevitably be destroyed."

There are, indeed, reasons for believing, not only, as I have already indicated, that the outburst in the sun was caused by the downfall of meteoric masses, but that those

masses were following in the train of a known comet, precisely as the November meteors follow in the train of Tempel's comet (II., 1866). For we know that November meteoric displays have been witnessed for five or six years after the passage of Tempel's comet, in its thirty-three year orbit, while the August meteoric displays have been witnessed fully one hundred and twenty years after the passage of their comet (II., 1862).\* Now only sixteen years before the solar outburst witnessed by Carrington and Hodgson, a magnificent comet had passed even closer to the sun than either Tempel's comet or the second comet of 1862 approached the earth's orbit. That was the famous comet of the year 1843. Many of us remember that wonderful object. I was but a child myself when it appeared, but I can well remember its amazing tail, which in March, 1843, stretched half-way across the sky.

"Of all the comets on record," says Sir J. Herschel, "that approached nearest the sun; indeed, it was at first supposed that it had actually grazed the sun's surface, but it proved to have just missed by an interval of not more than 80,000 miles—about a third of the distance of the moon from the earth, which (in such a matter) is a very close shave indeed to get clear off."

We can well believe that the two meteors which produced the remarkable outburst of 1859 may have been stragglers from the main body following after that glorious comet. I do not insist upon the connection. In fact, I rather incline to the belief that the disturbance in 1859, occurring as it did about the time of maximum sun-spot frequency, was caused by meteors following in the train of some as yet undiscovered comet, circuiting the sun in about eleven years, the spots

\* It may seem strange to say that one hundred and twenty years after the passage of a comet which last passed in 1862, and was then first discovered, August meteors have been seen. But in reality, as we know the period of that comet to be about one hundred and thirty years, we know that the displays of the years 1840, 1841, etc., to 1850, must have followed the preceding passage by about that interval of time.

themselves being, I believe, due in the main to meteoric downfalls. There is greater reason for believing that the great sun-spot which appeared in June, 1843, was caused by the comet which three months before had grazed the sun's surface. As Professor Kirkwood, of Bloomington, Indiana, justly remarks, had this comet approached a little nearer, the resistance of the solar atmosphere would probably have brought the comet's entire mass to the solar surface. Even at its actual distance, it must have produced considerable atmospheric disturbance. But the recent discovery that a number of comets are associated with meteoric matter travelling in nearly the same orbits, suggests the inquiry whether an enormous meteorite following in the comet's train, and having a somewhat less perihelion distance, may not have been precipitated upon the sun, thus producing the great disturbance observed so shortly after the comet's perihelion passage.

Let us consider now the evidence obtained from the star in Cygnus, noting especially in what points it resembles, and in what points it differs from, the evidence afforded by the star in the Crown.

The new star was first seen by Professor Schmidt at a quarter to six on the evening of November 24. It was then shining as a star of the third magnitude, in the constellation of the Swan, not very far from the famous but faint star 61 Cygni—which first of all the stars in the northern heavens had its distance determined by astronomers. The three previous nights had unfortunately been dark; but Schmidt is certain that on November 20 the star was not visible. At midnight, November 24, its light was very yellow, and it was somewhat brighter than the well-known star Eta Pegasi, which marks the forearm of the Flying Horse. Schmidt sent news of the discovery to Leverrier, at Paris; but neither he nor Leverrier telegraphed the news, as they should have done, to Greenwich, Berlin, or the United States. Many precious opportunities for observing the spectrum of the new-comer at the time of its greatest brilliancy were thus lost.

The observers at Paris did their best to observe the spectrum of the star and the all-important changes in the spectrum. But they had unfavourable weather. It was not till December 2 that the star was observed at Paris, by which time the colour, which had been very yellow on November 24, had become "greenish, almost blue." The star had also then sunk from the third to far below the fourth magnitude. It is seldom that science has to regret a more important loss of opportunity than this. What we want specially to know is the nature of the spectrum given by this star when its light was yellow ; and this we can now never know. Nor are the outbursts of new stars so common that we may quickly expect another similar opportunity, even if any number of other new stars should present the same series of phenomena as the star in Cygnus.

On December 2, the spectrum, as observed by M. Cornu, consisted almost entirely of bright lines. On December 5, he determined the position of these lines, though clouds still greatly interfered with his labours. He found three bright lines of hydrogen, the strong double sodium line in the orange-yellow, the triple magnesium line in the yellow-green, and two other lines—one of which seemed to agree exactly in position with a bright line belonging to the solar corona. All these lines were shining upon the rainbow-tinted background of the spectrum, which was relatively faint. He drew the conclusion that in chemical constitution the atmosphere of the new star was constituted exactly like the solar sierra.

Herr Vögel's observations commenced on December 5, and were continued at intervals until March 10, when the star had sunk to below the eighth magnitude.

Vögel's earlier observations agreed well with Cornu's. He remarks, however, that Cornu's opinion as to the exact resemblance of the chemical constitution of the star's atmosphere with that of the sierra is not just, for both Cornu and himself noticed one line which did not correspond with any line belonging to the solar sierra ; and this line

eventually became the brightest line of the whole spectrum. Comparing his own observations with those of Cornu, Vögel points out that they agree perfectly with regard to the presence of the three hydrogen lines, and that of the brightest line of the air spectrum (belonging to nitrogen), —which is the principal line of the spectrum of nebulæ. This is the line which has no analogue in the spectrum of the sierra.

We have also observations by F. Secchi, at Rome, Mr. Copeland, at Dunecht, and Mr. Backhouse, of Sunderland, all agreeing in the main with the observations made by Vögel and Cornu. In particular, Mr. Backhouse observed, as Vögel had done, that whereas in December the greenish-blue line of hydrogen, F, was brighter than the nitrogen line (also in the green-blue, but nearer the red end than F), on January 6 the nitrogen line was the brightest of all the lines in the spectrum of the new star.

Vögel, commenting on the results of his observations up to March 10, makes the following interesting remarks (I quote, with slight verbal alterations, from a paraphrase in a weekly scientific journal) :—“A stellar spectrum with *bright* lines is always a highly interesting phenomenon for any one acquainted with stellar spectrum analysis, and well worthy of deep consideration. Although in the chromosphere (sierra) of our sun, near the limb, we see numerous bright lines, yet only dark lines appear in the spectrum whenever we produce a small star-like image of the sun, and examine it through the spectroscope. It is generally believed that the bright lines in some few star-spectra result from gases which break forth from the interior of the luminous body, the temperature of which is higher than that of the surface of the body—that is, the phenomenon is the same sometimes observed in the spectra of solar spots, where incandescent hydrogen rushing out of the hot interior becomes visible above the cooler spots through the hydrogen lines turning bright. But this is not the only possible explanation. We may also suppose that the atmo-

sphere of a star, consisting of incandescent gases, as is the case with our own sun, is on the whole cooler than the nucleus, but with regard to the latter is extremely large. I cannot well imagine how the phenomenon can last for any long period of time if the former hypothesis be correct. The gas breaking forth from the hot interior of the body will impart a portion of its heat to the surface of the body, and thus raise the temperature of the latter; consequently, the difference of temperature between the incandescent gas and the surface of the body will soon be insufficient to produce bright lines; and these will disappear from the spectrum. This view applies perfectly to stars which suddenly appear and soon disappear again, or at least increase considerably in intensity—that is, it applies perfectly to so-called new stars in the spectra of which bright lines are apparent, *if* the hypothesis presently to be mentioned is admitted for their explanation. For a more stable state of things the second hypothesis seems to be far better adapted. Stars like Beta Lyræ, Gamma Cassiopeiae, and others, which show the hydrogen lines and the sierra D line bright on a continuous spectrum, with only slight changes of intensity, possess, according to this theory, atmospheres very large relatively to their own volume—the atmospheres consisting of hydrogen and that unknown element which produces the D line.\* With regard to the new star, Zöllner, long before the progress lately made in stellar physics by means of spectrum analysis, deduced from Tycho's observations of the star called after him, that on the surface of a star, through the constant emission of heat, the products of cooling, which in the case of our sun we call sun-spots, accumulate: so that finally the whole surface of the body is covered with a colder stratum, which gives

\* The D line, properly speaking, as originally named by Fraunhofer, belongs to sodium. The line spoken of above as the sierra D line is one close by the sodium line, and mistaken for it when first seen in the spectrum of the coloured prominences as a bright line. It does not appear as a dark line in the solar spectrum.

much less light or none at all. Through a sudden and violent tearing up of this stratum, the interior incandescent materials which it encloses must naturally break forth, and must in consequence, according to the extent of their eruption, cause larger or smaller patches of the dark envelope of the body to become luminous again. To a distant observer such an eruption from the hot and still incandescent interior of a heavenly body must appear as the sudden flashing-up of a new star. That this evolution of light may under certain conditions be an extremely powerful one, could be explained by the circumstance that all the chemical compounds which, under the influence of a lower temperature, had already formed upon the surface, are again decomposed through the sudden eruption of these hot materials ; and that this decomposition, as in the case of terrestrial substances, takes place under evolution of light and heat. Thus the bright flashing-up is not only ascribed to the parts of the surface which through the eruption of the incandescent matter have again become luminous, but also to a simultaneous process of combustion, which is initiated through the colder compounds coming into contact with the incandescent matter."

Vögel considers that Zöllner's hypothesis has been confirmed in its essential points by the application of spectrum analysis to the stars. We can recognize from the spectrum different stages in the process of cooling, and in some of the fainter stars we perceive indeed that chemical compounds have already formed, and still exist. As to new stars, again, says Vögel, Zöllner's theory seems in nowise contradicted "by the spectral observations made on the two new stars of 1866 and 1876. The bright continuous spectrum, and the bright lines only slightly exceeding it at first" (a description, however, applying correctly only to the star of 1876), "could not be well explained if we only suppose a violent eruption from the interior, which again rendered the surface wholly or partially luminous ; but are easily explained if we suppose that the quantity of light is considerably augmented through

a simultaneous process of combustion. If this process is of short duration, then the continuous spectrum, as was the case with the new star of 1876, will very quickly decrease in intensity down to a certain limit, while the bright lines in the spectrum, which result from the incandescent gases that have emanated in enormous quantities from the interior, will continue for some time."

It thus appears that Herr Vögel regarded the observations which had been made on this remarkable star up to March 10 as indicating that first there had been an outburst of glowing gaseous matter from the interior, producing the part of the light which gave the bright lines indicative of gaseity, and that then there had followed, as a consequence, the combustion of a portion of the solid and relatively cool crust, causing the continuous part of the spectrum. We may compare what had taken place, on this hypothesis, with the outburst of intensely hot gases from the interior of a volcanic crater, and the incandescence of the lips of the crater in consequence of the intense heat of the out-rushing gases. Any one viewing such a crater from a distance, with a spectroscope, would see the bright lines belonging to the out-rushing gases superposed upon the continuous spectrum due to the crater's burning lips. Vögel further supposes that the burning parts of the star soon cooled, the majority of the remaining light (or at any rate the part of the remaining light spectroscopically most effective) being that which came from the glowing gases which had emanated in vast quantities from the star's interior.

"The observations of the spectrum show, beyond doubt," he says, "that the decrease in the light of the star corresponds with the cooling of its surface. The violet and blue parts decreased more rapidly in intensity than the other parts; and the absorption-bands which crossed the spectrum have gradually become darker and darker."

The reasoning, however, if not altogether unsatisfactory, is by no means so conclusive as Herr Vögel appears to think. It is not clear how the incandescent portion of the surface

could possibly cool in any great degree while enormous quantities of gas more intensely heated (by the hypothesis) remained around the star. The more rapid decrease in the violet and blue parts of the spectrum than in the red and orange is explicable as an effect of absorption, at least as readily as by the hypothesis that burning solid or liquid matter had cooled. Vögel himself could only regard the other bands which crossed the spectrum as absorption-bands. And the absorption of light from the continuous spectrum in these parts (that is, not where the bright lines belonging to the gaseous matter lay) could not possibly result from absorption produced by those gases. If other gases were in question, gases which, by cooling with the cooling surface, had become capable of thus absorbing light from special parts of the spectrum, how is it that before, when these gases were presumably intensely heated, they did not indicate their presence by bright bands? Bright bands, indeed, were seen, which eventually faded out of view, but these bright bands did not occupy the position where, later on, absorption-bands appeared.

The natural explanation of what had thus far been observed is different from that advanced by Vögel, though we must not assume that because it is the natural, it is necessarily the true explanation. It is this—that the source of that part of the star's light which gave the bright-line spectrum, or the spectrum indicative of gaseity, belongs to the normal condition of the star, and not to gases poured forth, in consequence of some abnormal state of things, from the sun's interior. We should infer naturally, though again I say not *therefore* correctly, that if a star spectroscope had been directed upon the place occupied by the new star before it began to shine with unusual splendour, the bright-line spectrum would have been observed. Some exceptional cause would then seem to have aroused the entire surface of the star to shine with a more intense brightness, the matter thus (presumably) more intensely heated being such as would give out the combined continuous and bright-line

spectrum, including the bright lines which, instead of fading out, shone with at least relatively superior brightness as the star faded from view. The theory that, on the contrary, the matter giving these more persistent lines was that whose emission caused the star's increase of lustre, seems at least not proven, and I would go so far as to say that it accords ill with the evidence.

The question, be it noted, is simply whether we should regard the kind of light which lasts longest in this star as it fades out of view as more probably belonging to the star's abnormal brightness or to its normal luminosity. It seems to me there can be little doubt that the persistence of this part of the star's light points to the latter rather than to the former view.

Let it also be noticed that the changes which had been observed thus far were altogether unlike those which had been observed in the case of the star in the Northern Crown, and therefore cannot justly be regarded as pointing to the same explanation. As the star in the Crown faded from view, the bright lines indicative of glowing hydrogen died out, and only the ordinary stellar spectrum remained. In the case of the star in the Swan, the part of the spectrum corresponding to stellar light faded gradually from view, and bright lines only were left, at least as conspicuous parts of the star's spectrum. So that whereas one orb seemed to have faded into a faint star, the other seemed fading out into a nebula—not merely passing into such a condition as to shine with light indicative of gaseity, but actually so changing as to shine with light of the very tints (or, more strictly, of the very wave-lengths) observed in all the gaseous nebulæ.

The strange eventful history of the new star in Cygnus did not end here, however. We may even say, indeed, that it has not ended yet. But another chapter can already be written.

Vögel ceased from observing the star in March, precisely when observation seemed to promise the most interesting results. At most other observatories, also, no observations

were made for about half a year. At the Dunecht Observatory \* pressure of work relating to Mars interfered with the prosecution of those observations which had been commenced early in the year. But on September 3, Lord Lindsay's 15-inch reflector was directed upon the star. A star was still shining where the new star's yellow lustre had been displayed in November, 1876; but now the star shone with a faint blue colour. Under spectroscopic examination, however, the light from this seeming blue star was found not to be starlight, properly speaking, at all. It formed no rainbow-tinted spectrum, but gave light of only a single colour. The single line now seen was that which at the time of Vögel's latest observation had become the strongest of the bright lines of the originally complex spectrum of the so-called new star. It is the brightest of the lines given by the gaseous nebulae. In fact, if nothing had been known about this body before the spectroscopic observation of September 3 was made, the inference from the spectrum given by the blue star would undoubtedly have been that the object is in reality a small nebula of the planetary sort, very similar to that one close by the pole of the ecliptic, which gave Huggins the first evidence of the gaseity of nebulae, but very much smaller. I would specially direct the reader's attention, in fact, to Huggins's account of his observation of that planetary nebula in the Dragon. "On August 19, 1864," he says, "I directed the telescope armed with the spectrum apparatus to this nebula. At first I suspected some derangement of the instrument had taken place, for no spectrum was seen, but only" a single line of light. "I then found that the light of this nebula, unlike any other extra-terrestrial light which had yet been subjected by me to prismatic analysis, was not composed of light of different refrangibilities, and therefore could not form a spectrum. A great part of the light from this nebula

\* Since this was written, I have learned that Mr. Backhouse, of Sunderland, announced similar results to those obtained at Dunecht, as seen a fortnight or so earlier.

is monochromatic, and after passing through the prisms remains concentrated in a bright line." A more careful examination showed that not far from the bright line was a much fainter line; and beyond this, again, a third exceedingly faint line was seen. The brightest of the three lines was a line of nitrogen corresponding in position with the brightest of the lines in the spectrum of our own air. The faintest corresponded in position with a line of hydrogen. The other has not yet been associated with a known line of any element. Besides the faint lines, Dr. Huggins perceived an exceedingly faint continuous spectrum on both sides of the group of bright lines; he suspected, however, that this faint spectrum was not continuous, but crossed by dark spaces. Later observations on other nebulæ induced him to regard this faint continuous spectrum as due to the solid or liquid matter of the nucleus, and as quite distinct from the bright lines into which nearly the whole of the light from the nebula is concentrated. The fainter parts of the spectrum of the gaseous nebulæ, in fact, correspond to those parts of the spectrum of the "new star" in Cygnus which last remained visible, before the light assumed its present monochromatic colour.

Now let us consider the significance of the evidence afforded by this discovery—not perhaps hoping at once to perceive the full meaning of the discovery, but endeavouring to advance as far as we safely can in the direction in which it seems to point.

We have, then, these broad facts: where no star had been known, an object has for a while shone with stellar lustre, in this sense, that its light gave a rainbow-tinted spectrum not unlike that which is given by a certain order of stars; this object has gradually parted with its new lustre, and in so doing the character of its spectrum has slowly altered, the continuous portion becoming fainter, and the chief lustre of the bright-line portion shifting from the hydrogen lines to a line which, there is every reason to believe, is absolutely identical with the nebula nitrogen line;

and lastly, the object has ceased to give any perceptible light, other than that belonging to this nitrogen line.

Now it cannot, I think, be doubted that, accompanying the loss of lustre in this orb, there has been a corresponding loss of heat. The theory that all the solid and liquid materials of the orb have been vaporized by intense heat, and that this vaporization has caused the loss of the star's light (as a lime-light might die out with the consumption of the lime, though the flame remained as hot as ever), is opposed by many considerations. It seems sufficient to mention this, that if a mass of solid matter, like a dead sun or planet, were exposed to an intense heat, first raising it to incandescence, and eventually altogether vaporizing its materials, although quite possibly the time of its intensest lustre might precede the completion of the vaporization, yet certainly so soon as the vaporization was complete, the spectrum of the newly vaporized mass would show multitudinous bright lines corresponding to the variety of material existing in the body. No known fact of spectroscopic analysis lends countenance to the belief that a solid or liquid mass, vaporized by intense heat, would shine thenceforth with monochromatic light.

Again, I think we are definitely compelled to abandon Vögel's explanation of the phenomena by Zöllner's theory. The reasons which I have urged above are not only strengthened severally by the change which has taken place in the spectrum of the new star since Vögel observed it, but an additional argument of overwhelming force has been introduced. If any one of the suns died out, a crust forming over its surface and this crust being either absolutely dark or only shining with very feeble lustre, the sun would still in one respect resemble all the suns which are spread over the heavens—it would show no visible disc, however great the telescopic power used in observing it. If the nearest of all the stars were as large, or even a hundred times as large, as Sirius, and were observed with a telescope of ten times greater magnifying power than any yet

directed to the heavens, it would appear only as a point of light. If it lost the best part of its lustre, it would appear only as a dull point of light. Now the planetary nebulae show discs, sometimes of considerable breadth. Sir J. Herschel, to whom and to Sir W. Herschel we owe the discovery and observation of nearly all these objects, remarks that "the planetary nebulae have, as their name imports, a near, in some instances a perfect, resemblance to planets, presenting discs round, or slightly oval, in some quite sharply terminated, in others a little hazy or softened at the borders. . . ." Among the most remarkable may be specified one near the Cross, whose light is about equal to that of a star just visible to the naked eye, "its diameter about twelve seconds, its disc circular or very slightly elliptic, and with a clear, sharp, well-defined outline, having exactly the appearance of a planet, with the exception of its colour, which is a fine and full blue, verging somewhat upon green." But the largest of these planetary nebulae, not far from the southernmost of the two stars called the Pointers, has a diameter of  $2\frac{2}{3}$  minutes of arc, "which, supposing it placed at a distance from us not greater than that of the nearest known star of our northern heavens, would imply a linear diameter seven times greater than that of the orbit of Neptune." The actual volume of this object, on this assumption, would exceed our sun's ten million million times. No one supposes that this planetary nebula, shining with a light indicative of gaseity, has a mass exceeding our sun's in this enormous degree. It probably has so small a mean density as not greatly to exceed, or perhaps barely to equal, our sun in mass. Now though the "new star" in Cygnus presented no measurable disc, and still shines as a mere blue point in the largest telescope, yet inasmuch as its spectrum associated it with the planetary and gaseous nebulae, which we know to be much larger bodies than the stars, it must be regarded, in its present condition, as a planetary nebula, though a small one; and since we cannot for a moment imagine that the monstrous planetary

nebulæ just described are bodies which once were suns, but whose crust has now become non-luminous, while around the crust masses of gas shine with a faint luminosity, so are we precluded from believing that this smaller member of the same family is in that condition.

It is conceivable (and the possibility must be taken into account in any attempt to interpret the phenomena of the new star) that when shining as a star, the new orb, so far as this unusual lustre was concerned, was of sunlike dimensions. For we cannot tell whether the surface which gave the strong light was less or greater than, or equal to, that which is now shining with monochromatic light. Very likely, if we had been placed where we could have seen the full dimensions of the planetary nebula as it at present exists, we should have found only its nuclear part glowing suddenly with increased lustre, which, after very rapidly attaining its maximum, gradually died out again, leaving the nebula as it had been before. But that the mass now shining with monochromatic light is, I will not say enormously large, but of exceedingly small mean density, so that it is enormously large compared with the dimensions it would have if its entire substance were compressed till it had the same mean density as our own sun, must be regarded as, to all intents and purposes, certain.

We certainly have not here, then, the case of a sun which has grown old and dead and dark save at the surface, but within whose interior fire has still remained, only waiting some disturbing cause to enable it for a while to rush forth. If we could suppose that in such a case there could be such changes as the spectroscope has indicated—that the bright lines of the gaseous outbursting matter would, during the earlier period of the outburst, show on a bright continuous background, due to the glowing lips of the opening through which the matter had rushed, but later would shine alone, becoming also fewer in number, till at last only one was left, —we should find ourselves confronted with the stupendous difficulty that that single remaining line is the bright line of

the planetary and other gaseous nebulae. Any hypothesis accounting for its existence in the spectrum of the faint blue starlike object into which the star in Cygnus has faded ought to be competent to explain its existence in the spectrum of those nebulae. But *this* hypothesis certainly does not so explain its existence in the nebular spectrum. The nebulae cannot be suns which have died out save for the light of gaseous matter surrounding them, for they are millions, or rather millions of millions, of times too large. If, for instance, a nebula, like the one above described as lying near the southernmost Pointer, were a mass of this kind, having the same mean density as the sun, and lying only at the distance of the nearest of the stars from us, then not only would it have the utterly monstrous dimensions stated by Sir J. Herschel, but it would in the most effective way perturb the whole solar system. With a diameter exceeding seven times that of the orbit of Neptune, it would have a volume, and therefore a mass, exceeding our sun's volume and mass more than eleven millions of millions of times. But its distance on this assumption would be only about two hundred thousand times the sun's, and its attraction reduced, as compared with his, on this account only forty thousand millions of times. So that its attraction on the sun and on the earth would be greater than his attraction on the earth, in the same degree that eleven millions are greater than forty thousand—or two hundred and seventy-five times. The sun, despite his enormous distance from such a mass, would be compelled to fall very quickly into it, unless he circuited (with all his family) around it in about one-sixteenth of a year, which most certainly he does not do. Nor would increasing the distance at which we assume the star to lie have any effect to save the sun from being thus perturbed, but the reverse. If we double for instance our estimate of the nebula's distance, we increase eightfold our estimate of its mass, while we only diminish its attraction on our sun fourfold on account of increased distance; so that now its attraction on our sun would be one-fourth its former

attraction multiplied by eight, or twice our former estimate. We cannot suppose the nebula to be much nearer than the nearest star. Again, we cannot suppose that the light of these gaseous nebulæ comes from some bright orb within them of only starlike apparent dimensions, for in that case we should constantly recognize such starlike nucleus, which is not the case. Moreover, the bright-line spectrum from one of these nebulæ comes from the whole nebula, as is proved by the fact that if the slit of the spectroscope be opened it becomes possible to see three spectroscopic images of the nebula itself, not merely the three bright lines.

So that, if we assume the so-called star in Cygnus to be now like other objects giving the same monochromatic spectrum—and this seems the only legitimate assumption—we are compelled to believe that the light now reaching us comes from a nebulous mass, not from the faintly luminous envelope of a dead sun. Yet, remembering that when at its brightest this orb gave a spectrum resembling in general characteristics that of other stars or suns, and closely resembling even in details that of stars like Gamma Cassiopeïæ, we are compelled by parity of reasoning to infer that when the so-called new star was so shining, the greater part of its light came from a sunlike mass. Thus, then, we are led to the conclusion that in the case of this body we have a nucleus or central mass, and that around this central mass there is a quantity of gaseous matter, resembling in constitution that which forms the bulk of the other gaseous nebulæ. The denser nucleus ordinarily shines with so faint a lustre that the continuous spectrum from its light is too faint to be discerned with the same spectroscopic means by which the bright lines of the gaseous portion are shown; and the gaseous portion ordinarily shines with so faint a lustre that its bright lines would not be discernible on the continuous background of a stellar spectrum. Through some cause unknown—possibly (as suggested in an article on the earlier history of this same star in my “Myths and Marvels of Astronomy”) the rush of a rich

and dense flight of meteors upon the central mass—the nucleus was roused to a degree of heat far surpassing its ordinary temperature. Thus for a time it glowed as a sun. At the same time the denser central portions of the nebulous matter were also aroused to intenser heat, and the bright lines which ordinarily (and certainly at present) would not stand out bright against the rainbow-tinted background of a stellar spectrum, showed brightly upon the continuous spectrum of the new star. Then as the rush of meteors upon the nucleus and on the surrounding nebulous matter ceased—if that be the true explanation of the orb's accession of lustre—or as the cause of the increase of brightness, whatever that cause may have been, ceased to act, the central orb slowly returned to its usual temperature, the nebulous matter also cooling, the continuous spectrum slowly fading out, the denser parts of the nebulous matter exercising also a selective absorption (explaining the bands seen in the spectrum at this stage) which gradually became a continuous absorption—that is, affected the entire spectrum. Those component gases, also, of the nebulous portion which had for a while been excited to sufficient heat to show their bright lines, cooled until their lines disappeared, and none remained visible except for a while the three usual nebular lines, and latterly (owing to still further cooling) only the single line corresponding to the monochromatic light of the fainter gaseous nebulae.

## *STAR-GROUPING, STAR-DRIFT, AND STAR-MIST.*

*A Lecture delivered at the Royal Institution on May 6, 1870.*

NEARLY a century has passed since the greatest astronomer the world has ever known—the Newton of observational astronomy, as he has justly been called by Arago—conceived the daring thought that he would gauge the celestial depths. And because in his day, as indeed in our own, very little was certainly known respecting the distribution of the stars, he was forced to found his researches upon a guess. He supposed that the stars, not only those visible to the naked eye, but all that are seen in the most powerful telescopes, are suns, distributed with a certain general uniformity throughout space. It is my purpose to attempt to prove that—as Sir Wm. Herschel was himself led to suspect during the progress of his researches—this guess was a mistaken one ; that but a small proportion of the stars can be regarded as real suns ; and that in place of the uniformity of distribution conceived by Sir Wm. Herschel, the chief characteristic of the sidereal system is *infinite variety*.

In order that the arguments on which these views are based may be clearly apprehended, it will be necessary to recall the main results of Sir. Wm. Herschel's system of star-grouping.

Directing one of his 20-feet reflectors to different parts of

the heavens, he counted the stars seen in the field of view. Assuming that the telescope really reached the limits of the sidereal system, it is clear that the number of stars seen in any direction affords a means of estimating the relative extension of the system in that direction, provided always that the stars are really distributed throughout the system with a certain approach to uniformity. Where many stars are seen, there the system has its greatest extension ; where few, there the limits of the system must be nearest to us.

Sir Wm. Herschel was led by this process of star-grouping to the conclusion that the sidereal system has the figure of a cloven disc. The stars visible to the naked eye lie far within the limits of this disc. Stars outside the relatively narrow limits of the sphere including all the visible stars, are separately invisible. But where the system has its greatest extension these orbs produce collectively the diffused light which forms the Milky Way.

<sup>1</sup> Sir John Herschel, applying a similar series of researches to the southern heavens, was led to a very similar conclusion. His view of the sidereal system differs chiefly in this respect from his father's, that he considered the stars within certain limits of distance from the sun to be spread less richly through space than those whose united lustre produces the milky light of the galaxy.

Now it is clear that if the supposition on which these views are based is just, the three following results are to be looked for.

In the first place, the stars visible to the naked eye would be distributed with a certain general uniformity over the celestial sphere ; so that if on the contrary we find certain extensive regions over which such stars are strewn much more richly than over the rest of the heavens, we must abandon Sir Wm. Herschel's fundamental hypothesis and all the conclusions which have been based upon it.

In the second place, we ought to find no signs of the aggregation of lucid stars into streams or clustering groups. If we should find such associated groups, we must abandon

the hypothesis of uniform distribution and all the conclusions founded on it.

Thirdly, and most obviously of all, the lucid stars ought not to be associated in a marked manner with the figure of the Milky Way. To take an illustrative instance. When we look through a glass window at a distant landscape we do not find that the specks in the substance of the glass seem to follow the outline of valleys, hills, trees, or whatever features the landscape may present. In like manner, regarding the sphere of the lucid stars as in a sense the window through which we view the Milky Way, we ought not to find these stars, which are so near to us, associated with the figure of the Milky Way, whose light comes from distances so enormously exceeding those which separate us from the lucid stars. Here again; then, if there should appear signs of such association, we must abandon the theory that the sidereal system is constituted as Sir Wm. Herschel supposed.

It should further be remarked that the three arguments derived from these relations are independent of each other. They are not as three links of a chain, any one of which being broken the chain is broken. They are as three strands of a triple cord. If one strand holds, the cord holds. It may be shown that all three are to be trusted.

It is not to be expected, however, that the stars as actually seen should exhibit these relations, since far the larger number are but faintly visible ; so that the eye would look in vain for the signs of law among them, even though law may be there. What is necessary is that maps should be constructed on a uniform and intelligible plan, and that in these maps the faint stars should be made bright, and the bright stars brighter.

The maps exhibited during this discourse [since published as my "Library Atlas"] have been devised for this purpose amongst others. There are twelve of them, but they overlap, so that in effect each covers a tenth part of the heavens. There is first a north-polar map, then five maps symmetrically placed around it ; again, there is a

south-polar map, and five maps symmetrically placed round that map ; and these five so fit in with the first five as to complete the enclosure of the whole sphere. In effect, every map of the twelve has five maps symmetrically placed around it and overlapping it.

Since the whole heavens contain but 5932 stars visible to the naked eye, each of the maps should contain on the average about 593 stars. But instead of this being the case, some of the maps contain many more than their just proportion of stars, while in others the number as greatly falls short of the average. One recognizes, by combining these indications, the existence of a roughly circular region, rich in stars, in the northern heavens, and of another, larger and richer, in the southern hemisphere.

To show the influence of these rich regions, it is only necessary to exhibit the numerical relations presented by the maps.

The north-polar map, in which the largest part of the northern rich region falls, contains no less than 693 lucid stars, of which upwards of 400 fall within the half corresponding to the rich region. Of the adjacent maps, two contain upwards of 500 stars, while the remaining three contain about 400 each. Passing to the southern hemisphere, we find that the south-polar map, which falls wholly within a rich region, contains no less than 1132 stars ! One of the adjacent maps contains 834 stars, and the four others exhibit numbers ranging from 527 to 595.

It is wholly impossible not to recognize so unequal a distribution as exhibiting the existence of special laws of stellar aggregation.

It is noteworthy, too, that the greater Magellanic cloud falls in the heart of the southern rich region. Were there not other signs that this wonderful object is really associated with the sidereal system, it might be rash to recognize this relation as indicating the existence of a physical connection between the Nubecula Major and the southern region rich in stars. Astronomers have indeed so long regarded the

Nubeculæ as belonging neither to the sidereal nor to the nebular systems, that they are not likely to recognize very readily the existence of any such connection. Yet how strangely perverse is the reasoning which has led astronomers so to regard these amazing objects. Presented fairly, that reasoning amounts simply to this: The Magellanic clouds contain stars and they contain nebulæ; therefore they are neither nebular nor stellar. Can perversity of reasoning be pushed further? Is not the obvious conclusion this, that since nebulæ and stars are *seen* to be intermixed in the Nubeculæ, the nebular and stellar systems form in reality but one complex system?

As to the existence of star-streams and clustering aggregations, we have also evidence of a decisive character. There is a well-marked stream of stars running from near Capella towards Monoceros. Beyond this lies a long dark rift altogether bare of lucid orbs, beyond which again lies an extensive range of stars, covering Gemini, Cancer, and the southern parts of Leo. This vast system of stars resembles a gigantic sidereal billow flowing towards the Milky Way as towards some mighty shore-line. Nor is this description altogether fanciful; since one of the most marked instances of star-drift presently to be adduced refers to this very region. These associated stars *are* urging their way towards the galaxy, and that at a rate which, though seemingly slow when viewed from beyond so enormous a gap as separates us from this system, must in reality be estimated by millions of miles in every year.

Other streams and clustering aggregations there are which need not here be specially described. But it is worth noticing that all the well-marked streams recognized by the ancients seem closely associated with the southern rich region already referred to. This is true of the stars forming the River Eridanus, the serpent Hydra, and the streams from the water-can of Aquarius. It is also noteworthy that in each instance a portion of the stream lies outside the rich region, the rest within it; while all the

streams which lie on the same side of the galaxy tend towards the two Magellanic clouds.

Most intimate signs of association between lucid stars and the galaxy can be recognized—(i.) in the part extending from Cygnus to Aquila; (ii.) in the part from Perseus to Monoceros; (iii.) over the ship Argo; and (iv.) near Crux and the feet of Centaurus.

Before proceeding to the subject of Star-drift, three broad facts may be stated. They are, I believe, now recognized for the first time, and seem decisive of the existence of special laws of distribution among the stars:—

First, the rich southern region, though covering but a sixth part of the heavens, contains one-third of all the lucid stars, leaving only two-thirds for the remaining five-sixths of the heavens.

Secondly, if the two rich regions and the Milky Way be considered as one part of the heavens, the rest as another, then the former part is three times as richly strewn with lucid stars as the second.

Thirdly, the southern hemisphere contains one thousand more lucid stars than the northern, a fact which cannot but be regarded as most striking when it is remembered that the total number of stars visible to ordinary eyesight in both hemispheres falls short of 6000.

Two or three years ago, the idea suggested itself to me that if the proper motions of the stars were examined, they would be found to convey clear information respecting the existence of variety of structure, and special laws of distribution within the sidereal system.

In the first place, the mere amount of a star's apparent motion must be regarded as affording a means of estimating the star's distance. The nearer a moving object is, the faster it will seem to move, and *vice versa*. Of course in individual instances little reliance can be placed on this indication; but by taking the average proper motions of a set of stars, a trustworthy measure may be obtained of their average distance, as compared with the average distance of another set.

For example, we have in this process the means of settling the question whether the apparent brightness of a star is indeed a test of relative nearness. According to accepted theories the sixth-magnitude stars are ten or twelve times as far off as those of the first magnitude. Hence their motions should, on the average, be correspondingly small. Now, to make assurance doubly sure, I divided the stars into two sets, the first including the stars of the 1st, 2nd, and 3rd, the second including those of the 4th, 5th, and 6th magnitude. According to accepted views, the average proper motion for the first set should be about five times as great as that for the second. I was prepared to find it about three times as great; that is, not so much greater as the accepted theories require, but still considerably greater. To my surprise, I found that the average proper motion of the brighter orders of stars is barely equal to that of the three lower orders.

This proves beyond all possibility of question that by far the greater number of the fainter orders of stars (I refer here throughout to lucid stars) owe their faintness not to vastness of distance, but to real relative minuteness.

To pass over a number of other modes of research, the actual mapping of the stellar motions, and the discovery of the peculiarity to which I have given the name of star-drift, remain to be considered.

In catalogues it is not easy to recognize any instances of community of motion which may exist among the stars, owing to the method in which the stars are arranged. What is wanted in this case (as in many others which yet remain to be dealt with) is the adoption of a plan by which such relations may be rendered obvious to the eye. The plan I adopted was to attach to each star in my maps a small arrow, indicating the amount and direction of that star's apparent motion in 36,000 years (the time-interval being purposely lengthened, as otherwise most of the arrows would have been too small to be recognized). When this was done, several well-marked instances of community of motion could immediately be recognized.

It is necessary to premise, however, that before the experiment was tried, there were reasons for feeling very doubtful whether it would succeed. A system of stars might really be drifting athwart the heavens, and yet the drift might be rendered unrecognizable through the intermixture of more distant or nearer systems having motions of another sort and seen accidentally in the same general direction.

This was found to be the case, indeed, in several instances. Thus the stars in the constellation Ursa Major, and neighbouring stars in Draco, exhibit two well-marked directions of drift. The stars  $\beta$ ,  $\gamma$ ,  $\delta$ ,  $\epsilon$ , and  $\zeta$  of the Great Bear, besides two companions of the last-named star, are travelling in one direction, with equal velocity, and clearly form one system. The remaining stars in the neighbourhood are travelling in a direction almost exactly the reverse. But even this relation, thus recognized in a region of diverse motions, is full of interest. Baron Mädler, the well-known German astronomer, recognizing the community of motion between  $\zeta$  Ursæ and its companions, calculated the cyclic revolution of the system to be certainly not less than 7000 years. But when the complete system of stars showing this motion is considered, we get a cyclic period so enormous, that not only the life of man, but the life of the human race, the existence of our earth, nay, even the existence of the solar system, must be regarded as a mere day in comparison with that tremendous cycle.

Then there are other instances of star-drift where, though two directions of motion are not intermixed, the drifting nature of the motion is not at once recognized, because of the various distances at which the associated stars lie from the eye.

A case of this kind is to be met with in the stars forming the constellation Taurus. It was here that Mädler recognized a community of motion among the stars, but he did not interpret this as I do. He had formed the idea that the whole of the sidereal system must be in motion

around some central point; and for reasons which need not here be considered, he was led to believe that in whatever direction the centre of motion may lie, the stars seen in that general direction would exhibit a community of motion. Then, that he might not have to examine the proper motions all over the heavens, he inquired in what direction (in all probability) the centre of motion may be supposed to lie. Coming to the conclusion that it must lie towards Taurus, he examined the proper motions in that constellation, and found a community of motion which led him to regard Alcyone, the chief star of the Pleiades, as the centre around which the sidereal system is moving. Had he examined further he would have found more marked instances of community of motion in other parts of the heavens, a circumstance which would have at once compelled him to abandon his hypothesis of a central sun in the Pleiades, or at least to lay no stress on the evidence derivable from the community of motion in Taurus.

Perhaps the most remarkable instance of star-drift is that observed in the constellations Gemini and Cancer. Here the stars seem to set bodily towards the neighbouring part of the Milky Way. The general drift in that direction is too marked, and affects too many stars, to be regarded as by any possibility referable to accidental coincidence.

It is worthy of note that if the community of star-drift should be recognized (or I prefer to say, *when* it is recognized), astronomers will have the means of determining the relative distances of the stars of a drifting system. For differences in the apparent direction and amount of motion can be due but to differences of distance and position, and the determination of these differences becomes merely a question of perspective.\*

Before long it is likely that the theory of star-drift will be subjected to a crucial test, since spectroscopic analysis affords the means of determining the stellar motions of

\* Here no account is taken of the motions of the stars within the system; such motions must ordinarily be minute compared with the common motion of the system.

recess or approach. The task is a very difficult one, but astronomers have full confidence that in the able hands of Mr. Huggins it will be successfully accomplished. I await the result with full confidence that it will confirm my views. (See pages 92-94 for the result.)

Turning to the subject of Star-mist, under which head I include all orders of nebulæ, I propose to deal with but a small proportion of the evidence I have collected to prove that none of the nebulæ are external galaxies. That evidence has indeed become exceedingly voluminous.

I shall dwell, therefore, on three points only.

First, as to the distribution of the nebulæ:—They are not spread with any approach to uniformity over the heavens, but are gathered into streams and clusters. The one great law which characterizes their distribution is an avoidance of the Milky Way and its neighbourhood. This peculiarity has, strangely enough, been regarded by astronomers as showing that there is no association between the nebulæ and the sidereal system. They have forgotten that marked contrast is as clear a sign of association as marked resemblance, and has always been so regarded by logicians.

Secondly, there are in the southern heavens two well-marked streams of nebulæ. Each of these streams is associated with an equally well-marked stream of stars. Each intermixed stream directs its course towards a Magellanic Cloud, one towards the Nubecula Minor, the other towards the Nubecula Major. To these great clusters they flow, like rivers towards some mighty lake. And within these clusters, which are doubtless roughly spherical in form, there are found intermixed in wonderful profusion, stars, star-clusters, and all the orders of nebulæ. Can these coincidences be regarded as accidental? And if not accidental, is not the lesson they clearly teach us this, that nebulæ form but portions of the sidereal system, associating themselves with stars on terms of equality (if one may so speak), even if single stars be not more important objects in the scale of

creation than these nebulous masses, which have been so long regarded as equalling, if not outvying, the sidereal system itself in extent?

The third point to which I wish to invite attention is the way in which in many nebulae stars of considerable relative brightness, and belonging obviously to the sidereal system, are so associated with nebulous masses as to leave no doubt whatever that these masses really cling around them. The association is in many instances far too marked to be regarded as the effect of accident.

Among other instances\* may be cited the nebula round the stars  $\zeta^1$  and  $\zeta^2$  in Orion. In this object two remarkable nebulous nodules centrally surround two double stars. Admitting the association here to be real (and no other explanation can reasonably be admitted), we are led to interesting conclusions respecting the whole of that wonderful nebulous region which surrounds the sword of Orion. We are led to believe that the other nebulae in that region are really associated with the fixed stars there; that it is not a mere coincidence, for instance, that the middle star in the belt of Orion is involved in nebula, or that the lowest star of the sword is similarly circumstanced. It is a legitimate inference from the evidence that all the nebulae in this region belong to one great nebulous group, which extends its branches to these stars. As a mighty hand, this nebulous region seems to gather the stars here into close association, showing us, in a way there is no misinterpreting, that these stars form one system.

The nebula around the strange variable star, Eta Argūs, is another remarkable instance of this sort. More than two years ago I ventured to make two predictions about this object. The first was a tolerably safe one. I expressed my belief that the nebula would be found to be gaseous. After Mr. Huggins's discovery that the great Orion nebula is gaseous, it was not difficult to see that the Argo nebula must

\* Eight pictures of nebulae were exhibited in illustration of this peculiarity.

be so too. At any rate, this has been established by Captain Herschel's spectroscopic researches. The other prediction was more venturesome. Sir John Herschel, whose opinion on such points one would always prefer to share, had expressed his belief that the nebula lies far out in space beyond the stars seen in the same field of view. I ventured to express the opinion that those stars are involved in the nebula. Lately there came news from Australia that Mr. Le Sueur, with the great reflector erected at Melbourne, has found that the nebula has changed largely in shape since Sir John Herschel observed it. Mr. Le Sueur accordingly expressed his belief that the nebula lies *nearer* to us than the fixed stars seen in the same field of view. More lately, however, he has found that the star Eta Argūs is shining with the light of burning hydrogen, and he expresses his belief that the star has consumed the nebulous matter near it. Without agreeing with this view, I recognize in it a proof that Mr. Le Sueur now considers the nebula to be really associated with the stars around it. My belief is that as the star recovers its brilliancy observation will show that the nebula in its immediate neighbourhood becomes brighter (*not* fainter through being consumed as fuel). In fact, I am disposed to regard the variations of the nebula as systematic, and due to orbital motions among its various portions around neighbouring stars.

As indicative of other laws of association bearing on the relations I have been dealing with, I may mention the circumstance that red stars and variable stars affect the neighbourhood of the Milky Way or of well-marked star-streams. The constellation Orion is singularly rich in objects of this class. It is here that the strange "variable" Betelgeux lies. At present this star shows no sign of variation, but a few years ago it exhibited remarkable changes. One is invited to believe that the star may have been carried by its proper motion into regions where there is a more uniform distribution of the material whence this orb recruits its fires. It may be that in the consideration of such causes of variation affecting

our sun in long past ages a more satisfactory explanation than any yet obtained may be found of the problem geologists find so perplexing—the former existence of a tropical climate in places within the temperate zone, or even near the Arctic regions.\*

It remains that I should exhibit the general results to which I have been led. It has seemed to many that my views tend largely to diminish our estimate of the extent of the sidereal system. The exact reverse is the case. According to accepted views there lie within the range of our most powerful telescopes millions of millions of suns. According to mine the primary suns within the range of our telescopes must be counted by tens of thousands, or by hundreds of thousands at the outside. What does this diminution of numbers imply but that the space separating sun from sun is enormously greater than accepted theories would permit? And this increase implies an enormous increase in the estimate we are to form of the vital energies of individual suns. For the vitality of a sun, if one may be permitted the expression, is measured not merely by the amount of matter over which it exercises control, but by the extent of space within which that matter is distributed. Take an orb a thousand times vaster than our sun, and spread over its surface an amount of matter exceeding a thousandfold the combined mass of all the planets of the solar system:—So far as living force is concerned, the result is—*nil*. But distribute that matter throughout a vast space all round the orb:—That orb becomes at once fit to be the centre of a host of dependent worlds. Again, according to accepted theories, when the astronomer has succeeded in resolving the milky light of a portion of the galaxy into stars, he has in that direction, at any rate, reached the limits of the sidereal system. According to my views, what he

\* Sir John Herschel long since pointed to the variation of our sun as a possible cause of such changes of terrestrial climate.

has really done has been but to analyze a definite aggregation of stars, a mere corner of that great system. Yet once more, according to accepted views, thousands and thousands of galaxies, external to the sidereal system, can be seen with powerful telescopes. If I am right, the external star-systems lie far beyond the reach of the most powerful telescope man has yet been able to construct, insomuch that perchance the nearest of the outlying galaxies may lie a million times beyond the range even of the mighty mirror of the great Rosse telescope.

But this is little. Wonderful as is the extent of the sidereal system as thus viewed, even more wonderful is its infinite variety. We know how largely modern discoveries have increased our estimate of the complexity of the planetary system. Where the ancients recognized but a few planets, we now see, besides the planets, the families of satellites; we see the rings of Saturn, in which minute satellites must be as the sands on the sea-shore for multitude; the wonderful zone of asteroids; myriads on myriads of comets; millions on millions of meteor-systems, gathering more and more richly around the sun, until in his neighbourhood they form the crown of glory which bursts into view when he is totally eclipsed. But wonderful as is the variety seen within the planetary system, the variety within the sidereal system is infinitely more amazing. Besides the single suns, there are groups and systems and streams of primary suns; there are whole galaxies of minor orbs; there are clustering stellar aggregations, showing every variety of richness, of figure, and of distribution; there are all the various forms of nebulæ, resolvable and irresolvable, circular, elliptical, and spiral; and lastly, there are irregular masses of luminous gas, clinging in fantastic convolutions around stars and star-systems. Nor is it unsafe to assert that other forms and variety of structure will yet be discovered, or that hundreds more exist which we may never hope to recognize.

But lastly, even more wonderful than the infinite variety

of the sidereal system, is its amazing vitality. Instead of millions of inert masses, we see the whole heavens instinct with energy—astir with busy life. The great masses of luminous vapour, though occupying countless millions of cubic miles of space, are moved by unknown forces like clouds before the summer breeze; star-mist is condensing into clusters; star-clusters are forming into suns; streams and clusters of minor orbs are swayed by unknown attractive energies; and primary suns singly or in systems are pursuing their stately path through space, rejoicing as giants to run their course, extending on all sides the mighty arm of their attraction, gathering from ever-new regions of space supplies of motive energy, to be transformed into the various forms of force—light and heat and electricity—and distributed in lavish abundance to the worlds which circle round them.

Truly may I say, in conclusion, that whether we regard its vast extent, its infinite variety, or the amazing vitality which pervades its every portion, the sidereal system is, of all the subjects man can study, the most imposing and the most stupendous. It is as a book full of mighty problems—of problems which are as yet almost untouched by man, of problems which it might seem hopeless for him to attempt to solve. But those problems are given to him for solution, and he *will* solve them, whenever he dares attempt to decipher aright the records of that wondrous volume.

## *MALLET'S THEORY OF VOLCANOES.*

THERE are few subjects less satisfactorily treated in scientific treatises than that which Humboldt calls the Reaction of the Earth's Interior. We find, not merely in the configuration of the earth's crust, but in actual and very remarkable phenomena, evidence of subterranean forces of great activity; and the problems suggested seem in no sense impracticable: yet no theory of the earth's volcanic energy has yet gained general acceptance. While the astronomer tells us of the constitution of orbs millions of times further away than our own sun, the geologist has hitherto been unable to give an account of the forces which agitate the crust of the orb on which we live.

The theory put forward respecting volcanic energy, however, by the eminent seismologist Mallet, promises not merely to take the place of all others, but to gain a degree of acceptance which has not been accorded to any theory previously enunciated. It is, in principle, exceedingly simple, though many of the details (into which I do not propose to enter) involve questions of considerable difficulty.

Let us, in the first place, consider briefly the various explanations which had been already advanced.

There was first the chemical theory of volcanic energy, the favourite theory of Sir Humphry Davy. It is possible to produce on a small scale nearly all the phenomena due to subterranean activity, by simply bringing together certain

substances, and leaving them to undergo the chemical changes due to their association. As a familiar instance of explosive action thus occasioned, we need only mention the results experienced when any one unfamiliar with the methods of treating lime endeavours over hastily to "slake" or "slack" it with water. Indeed, one of the strong points of the chemical theory consisted in the circumstance that volcanoes only occur where water can reach the subterranean regions—or, as Mallet expresses it, that "without water there is no volcano." But the theory is disposed of by the fact, now generally admitted, that the chemical energies of our earth's materials were almost wholly exhausted before the surface was consolidated.

Another inviting theory is that according to which the earth is regarded as a mere shell of solid matter surrounding a molten nucleus. There is every reason to believe that the whole interior of the earth is in a state of intense heat; and if the increase of heat with depth (as shown in our mines) is supposed to continue uniformly, we find that at very moderate depths a degree of heat must prevail sufficient to liquefy any known solids under ordinary conditions. But the conditions under which matter exists a few miles only below the surface of the earth are not ordinary. The pressure enormously exceeds any which our physicists can obtain experimentally. The ordinary distinction between solids and liquids cannot exist at that enormous pressure. A mass of cold steel could be as plastic as any of the glutinous liquids, while the structural change which a solid undergoes in the process of liquefying could not take place under such pressure even at an enormously high temperature. It is now generally admitted that if the earth really has a molten nucleus, the solid crust must, nevertheless, be far too thick to be in any way disturbed by changes affecting the liquid matter beneath.

Yet another theory has found advocates. The mathematician Hopkins, whose analysis of the molten-nucleus theory was mainly effective in showing that theory to be un-

tenable, suggested that there may be isolated subterranean lakes of fiery matter, and that these may be the true seat of volcanic energy. But such lakes could not maintain their heat for ages, if surrounded (as the theory requires) by cooler solid matter, especially as the theory also requires that water should have access to them. It will be observed also that none of the theories just described affords any direct account of those various features of the earth's surface—mountain ranges, table-lands, volcanic regions, and so on—which are undoubtedly due to the action of subterranean forces. The theory advanced by Mr. Mallet is open to none of these objections. It seems, indeed, competent to explain all the facts which have hitherto appeared most perplexing.

It is recognized by physicists that our earth is gradually parting with its heat. As it cools it contracts. Now if this process of contraction took place uniformly, no subterranean action would result. But if the interior contracts more quickly than the crust, the latter must in some way or other force its way down to the retreating nucleus. Mr. Mallet shows that the hotter internal portion must contract faster than the relatively cool crust; and then he shows that the shrinkage of the crust is competent to occasion all the known phenomena of volcanic action. In the distant ages when the earth was still fashioning, the shrinkage produced the *irregularities of level* which we recognize in the elevation of the land and the depression of the ocean-bed. Then came the period when as the crust shrank it formed *corrugations*, in other words, when the foldings and elevations of the somewhat thickened crust gave rise to the mountain-ranges of the earth. Lastly, as the globe gradually lost its extremely high temperature, the continuance of the same process of shrinkage led no longer to the formation of ridges and table-lands, but to local crushing-down and dislocation. This process is still going on, and Mr. Mallet not only recognizes here the origin of earthquakes, and of the changes of level now in progress, but the true cause of

volcanic heat. The modern theory of heat as a form of motion here comes into play. As the solid crust closes in upon the shrinking nucleus, the work expended in crushing down and dislocating the parts of the crust is transformed into heat, by which, at the places where the process goes on with greatest energy, "the materials of the rock so crushed and of that adjacent to it are heated even to fusion. The access of water to such points determines volcanic eruption."

Now all this is not mere theorising. Mr. Mallet does not come before the scientific world with an ingenious speculation, which may or may not be confirmed by observation and experiment. He has measured and weighed the forces of which he speaks. He is able to tell precisely what proportion of the actual energy which must be developed as the earth contracts is necessary for the production of observed volcanic phenomena. It is probable that nine-tenths of those who have read these lines would be disposed to think that the contraction of the earth must be far too slow to produce effects so stupendous as those which we recognize in the volcano and the earthquake. But Mr. Mallet is able to show, by calculations which cannot be disputed, that less than one-fourth of the heat at present annually lost by the earth is sufficient to account for the total annual volcanic action, according to the best data at present in our possession.

As I have said, I do not propose to follow out Mr. Mallet's admirable theory into all its details. I content myself with pointing out how excellently it accounts for certain peculiarities of the earth's surface configuration. Few that have studied carefully drawn charts of the chief mountain-ranges can have failed to notice that the arrangement of these ranges does not accord with the idea of upheaval through the action of internal forces. But it will be at once recognized that the aspect of the mountain-ranges accords exactly with what would be expected to result from such a process of contraction as Mr. Mallet

has indicated. The shrivelled skin of an apple affords no inapt representation of the corrugated surface of our earth, and according to the new theory, the shrivelling of such a skin is precisely analogous to the processes at work upon the earth when mountain-ranges were being formed. Again, there are few students of geology who have not found a source of perplexity in the foldings and overlappings of strata in mountainous regions. No forces of upheaval seem competent to produce this arrangement. But by the new theory this feature of the earth's surface is at once explained ; indeed, no other arrangement could be looked for.

It is worthy of notice that Mr. Mallet's theory of Volcanic energy is completely opposed to ordinary ideas respecting earthquakes and volcanoes. We have been accustomed vaguely to regard these phenomena as due to the eruptive outbursting power of the earth's interior ; we shall now have to consider them as due to the subsidence and shrinkage of the earth's exterior. Mountains have not been upheaved, but valleys have sunk down. And in another respect the new theory tends to modify views which have been generally entertained in recent times. Our most eminent geologists have taught that the earth's internal forces may be as active now as in the epochs when the mountain-ranges were formed. But Mr. Mallet's theory tends to show that the volcanic energy of the earth is a declining force. Its chief action had already been exerted when mountains began to be formed ; what remains now is but the minutest fraction of the volcanic energy of the mountain-forming era ; and each year, as the earth parts with more and more of its internal heat, the sources of her subterranean energy are more and more exhausted. The thought once entertained by astronomers that the earth might explode like a bomb, her scattered fragments producing a ring of bodies resembling the zone of asteroids, seems further than ever from probability ; if ever there was any danger of such a catastrophe, the danger has long since passed away.

## *TOWARDS THE NORTH POLE.*

THE Arctic Expedition which returned to our shores in the autumn of 1876 may be regarded as having finally decided the question whether the North Pole of the earth is accessible by the route through Smith's Sound—a route which may conveniently and properly be called the American route. Attacks may hereafter be made on the Polar fastness from other directions ; but it is exceedingly unlikely that this country, at any rate, will again attempt to reach the Pole along the line of attack followed by Captain Nares's expedition. I may be forgiven, perhaps, for regarding Arctic voyages made by the seamen of other nations as less likely to be successful than those made by my own countrymen. It is not mere national prejudice which suggests this opinion. It is the simple fact that hitherto the most successful approaches towards both the Northern and the Southern Poles have been made by British sailors. Nearly a quarter of a century has passed since Sir E. Parry made the nearest approach to the North Pole recorded up to that time ; and although, in the interval between Parry's expedition and Nares's, no expedition had been sent out from our shores with the object of advancing towards the Pole, while America, Sweden, Russia, and Germany sent out several, Parry's attempt still remained unsurpassed and unequalled. At length it has been surpassed, but it has been by his own countrymen. In like manner, no nation has yet succeeded in approaching the Antarctic Pole so nearly, within many miles, as did Captain Sir J. C. Ross in 1844. Considering

these circumstances, and remembering the success which rewarded the efforts of Great Britain in the search for the North-West Passage, it cannot be regarded as national prejudice to assert that events indicate the seamen of this country as exceptionally fitted to contend successfully against the difficulties and the dangers of Arctic exploration. Should England, then, give up the attempt to reach the North Pole by way of Smith's Sound and its northerly prolongation, it may fairly be considered unlikely that the Pole will ever be reached in that direction.

It may be well to examine the relative probable chances of success along other routes which have either not been so thoroughly tried, or have been tried under less favourable conditions.

Passing over the unfortunate expedition under Hugh Willoughby in 1553, the first attempt to penetrate within the Polar domain was made by Henry Hudson in 1607. The route selected was one which many regard (and I believe correctly) as the one on which there is the best chance of success; namely, the route across the sea lying to the west of Spitzbergen. That Hudson, in the clumsy galleons of Elizabeth's time, should have penetrated to within eight degrees and a half of the Pole, or to a distance only exceeding Nares's nearest approach by about 130 miles, proves conclusively, we think, that with modern ships, and especially with the aid of steam, this route might be followed with much better prospect of success than that which was adopted for Nares's expedition. If the reader will examine a map of the Arctic regions he will find that the western shores of Spitzbergen and the north-eastern shores of Greenland, as far as they have been yet explored, are separated by about 33 degrees of longitude, equivalent on the 80th parallel of latitude to about 335 miles. Across the whole breadth of this sea Arctic voyagers have attempted to sail northwards beyond the 80th parallel, but no one has yet succeeded in the attempt except on the eastern side of that sea. It was here that Hudson—fortunately for him—directed his attack;

and he passed a hundred miles to the north of the 80th parallel, being impeded and finally stopped by the packed ice around the north-western shores of Spitzbergen.

Let us consider the fortunes of other attempts which have been made to approach the Pole in this direction.

In 1827 Captain (afterwards Sir Edward) Parry, who had already four times passed beyond the Arctic Circle—viz., in 1818, 1819, 1821–23, and 1824–25—made an attempt to reach the North Pole by way of Spitzbergen. His plan was to follow Hudson's route until stopped by ice; then to leave his ship, and cross the ice-field with sledges drawn by Esquimaux dogs, and, taking boats along with the party, to cross whatever open water they might find. In this way he succeeded in reaching latitude  $82^{\circ} 45'$  north, the highest ever attained until Nares's expedition succeeded in crossing the 83rd parallel. Parry found that the whole of the ice-field over which his party were laboriously travelling northwards was being carried bodily southwards, and that at length the distance they were able to travel in a day was equalled by the southerly daily drift of the ice-field, so that they made no real progress. He gave up further contest, and returned to his ship the *Hecla*.

It is important to inquire whether the southerly drift which stopped Parry was due to northerly winds or to a southerly current; and if to the latter cause, whether this current probably affects the whole extent of the sea in which Parry's ice-field was drifting. We know that his party were exposed, during the greater part of their advance from Spitzbergen, to northerly winds. Now the real velocity of these winds must have been greater than their apparent velocity, because the ice-field was moving southwards. Had this not been the case, or had the ice-field been suddenly stopped, the wind would have seemed stronger; precisely as it seems stronger to passengers on board a sailing vessel when, after being before the wind for a time, she is brought across the wind. The ice-field was clearly travelling before the wind, but not nearly so fast as the wind; and therefore there is

good reason for believing that the motion of the ice-field was due to the wind alone. If we suppose this to have been really the case, then, as there is no reason for believing that northerly winds prevail uniformly in the Arctic regions, we must regard Parry's defeat as due to mischance. Another explorer might have southerly instead of northerly winds, and so might be assisted instead of impeded in his advance towards the Pole. Had this been Parry's fortune, or even if the winds had proved neutral, he would have approached nearer to the Pole than Nares. For Parry reckoned that he had lost more than a hundred miles by the southerly drift of the ice-field, by which amount at least he would have advanced further north. But that was not all ; for there can be little doubt that he would have continued his efforts longer but for the Sisyphean nature of the struggle. It is true he was nearer home when he turned back than he would have been but for the drift, and one of his reasons for turning back was the consideration of the distance which his men had to travel in returning. But he was chiefly influenced (so far as the return journey was concerned) by the danger caused by the movable nature of the ice-field, which might at any time begin to travel northwards, or eastwards, or westwards.

If we suppose that not the wind but Arctic currents carried the ice-field southwards, we must yet admit the probability—nay, almost the certainty—that such currents are only local, and occupy but a part of the breadth of the North Atlantic seas in those high latitudes. The general drift of the North Atlantic surface-water is unquestionably not towards the south but towards the north ; and whatever part we suppose the Arctic ice to perform in regulating the system of oceanic circulation—whether, with Carpenter, we consider the descent of the cooled water as the great moving cause of the entire system of circulation, or assign to that motion a less important office (which seems to me the juster opinion)—we must in any case regard the Arctic seas as a region of surface indraught. The current flowing from those seas, which caused (on the hypothesis we are for the moment

adopting) the southwardly motion of Parry's ice-field, must therefore be regarded as in all probability an exceptional phenomenon of those seas. By making the advance from a more eastwardly or more westwardly part of Spitzbergen, a northerly current would probably be met with ; or rather, the motion of the ice-field would indicate the presence of such a current, for I question very much whether open water would anywhere be found north of the 83rd parallel. In that case, a party might advance in one longitude and return in another, selecting for their return the longitude in which (always according to our present hypothesis that currents caused the drift) Parry found that a southerly current underlay his route across the ice. On the whole, however, it appears to me more probable that winds, not currents, caused the southerly drift of Parry's ice-field.

In 1868, a German expedition, under Captain Koldewey, made the first visit to the seas west of Spitzbergen in a steamship, the small but powerful screw steamer *Germania* (126 tons), advancing northwards a little beyond the 81st parallel. But this voyage can scarcely be regarded as an attempt to approach the Pole on that course ; for Koldewey's instructions were, "to explore the eastern coast of Greenland northwards ; and, if he found success in that direction impossible, to make for the mysterious Island of Gilles on the east of Spitzbergen."

Scoresby in 1806 had made thus far the most northerly voyage in a ship on Hudson's route, but in 1868 a Swedish expedition attained higher latitudes than had ever or have ever been reached by a ship in that direction. The steamship *Sofia*, strongly built of Swedish iron, and originally intended for winter voyages in the Baltic, was selected for the voyage. Owing to a number of unfortunate delays, it was not until September, 1868, that the *Sofia* reached the most northerly part of her journey, attaining a point nearly fifteen miles further north than Hudson had reached. To the north broken ice was still found, but it was so closely packed that not even a boat could pass through. Two

months earlier in the season the voyagers might have waited for a change of wind and the breaking up of the ice ; but in the middle of September this would have been very dangerous. The temperature was already sixteen degrees below the freezing-point, and there was every prospect that in a few weeks, or even days, the seas over which they had reached their present position would be icebound. They turned back from that advanced position ; but, with courage worthy of the old Vikings, they made another attack a fortnight later. They were foiled again, as was to be expected, for by this time the sun was already on the wintry side of the equator. They had, indeed, a narrow escape from destruction. “An ice-block with which they came into collision opened a large leak in the ship’s side, and when, after great exertions, they reached the land, the water already stood two feet over the cabin floor.”\*

On the western side of the North Atlantic Channel—so to term the part lying between Greenland and Spitzbergen—the nearest approach towards the Pole was made by the Dutch in 1670, nearly all the more recent attempts to reach high northern latitudes in this direction having hitherto ended in failure more or less complete.

We have already seen that Captain Koldewey was charged to explore the eastern coast of Greenland in the *Germania* in 1868. In 1869 the *Germania* was again despatched under his command from Bremerhaven, in company with the *Hansa*, a sailing vessel. Lieutenant Payer and other Austrian *savants* accompanied Captain Koldewey. The attack was again made along the eastern shores of Greenland. As far as the 74th degree the two vessels kept company ; but at this stage it happened unfortunately that a signal from the *Germania* was misinterpreted,

\* During these journeys the Atlantic was sounded, and Scoresby’s estimate of the enormous depth of the Atlantic to the north-west of Spitzbergen was fully confirmed, the line indicating a depth of more than two miles. It was found also that Spitzbergen is connected with Norway by a submarine bank.

and the *Hansa* left her. Soon after, the *Hansa* was crushed by masses of drifting ice, and her crew and passengers took refuge on an immense ice-floe seven miles in circumference. Here they built a hut, which was in its turn crushed. Winds and currents carried their icy home about, and at length broke it up. Fortunately they had saved their boats, and were able to reach Friedrichsthal, a missionary station in the south of Greenland, whence they were conveyed to Copenhagen in September, 1870. Returning to the *Germania*, we find that she had a less unfortunate experience. She entered the labyrinth of sinuous fjords, separated by lofty promontories, and girt round by gigantic glaciers, which characterize the eastern coast of Greenland to the north of Scoresby Sound. In August the channels by which she had entered were closed, and the *Germania* was imprisoned. So soon as the ice would bear them, Koldewey and his companions made sledging excursions to various points around their ship. But in November the darkness of the polar winter settled down upon them, and these excursions ceased. The polar winter of 1869-70 was "characterized by a series of violent northerly tempests, one of which continued more than 100 hours, with a velocity (measured by the anemometer) of no less than sixty miles an hour"—a velocity often surpassed, indeed, but which must have caused intense suffering to all who left the shelter of the ship; for it is to be remembered that the air which thus swept along at the rate of a mile a minute was the bitter air of the Arctic regions. The thermometer did not, however, descend lower than 26° below zero, or 58° below the freezing-point—a cold often surpassed in parts of the United States. I have myself experienced a cold of more than 30° below zero, at Niagara. "With proper precautions as regards shelter and clothing," proceeds the narrative, "even extreme cold need not cause great suffering to those who winter in such regions. One of the worst things to be endured is the physical and moral weariness of being cut off from ex-

ternal observations during the long night of some ninety days, relieved only by the strange Northern Lights. The ice accumulates all round with pressure, and assumes peculiar and fantastic forms, emitting ever and anon ominous noises. Fortunately, the *Germania* lay well sheltered in a harbour opening southwards, and, being protected by a rampart of hills on the north, was able to resist the shock of the elements. The sun appearing once more about the beginning of February, the scientific work of exploration began. . . . The pioneers of the *Germania* advanced as far as the 77th degree of latitude, in longitude 18° 50' west from Greenwich. There was no sign of an open sea towards the Pole. *Had it not been for want of provisions, the party could have prolonged their sledge journey indefinitely.* The bank of ice, without remarkable protuberances, extends to about two leagues from the shore, which from this extreme point seems to trend towards the north-west, where the view was bounded by lofty mountains." As the expedition was only equipped for one winter, it returned to Europe in September, 1870, without having crossed the 78th parallel of north latitude.

Captain Koldewey was convinced, by the results of his exploration, that there is no continuous channel northwards along the eastern coast of Greenland. It does not seem to me that his expedition proved this beyond all possibility of question. Still, it seems clear that the eastern side of the North Atlantic is less suited than the western for the attempt to reach the North Pole. The prevailing ocean-currents are southerly on that side, just as they are northerly on the western side. The cold also is greater, the lines of equal temperature lying almost exactly in the direction of the channel itself—that is, nearly north and south—and the cold increasing athwart that direction, towards the west. The nearer to Greenland the greater is the cold.\*

\* It is far from improbable that a change has taken place in the climate of the part of the Arctic regions traversed by Koldevey; for the Dutch seem readily to have found their way much further north two

The next route to be considered in order of time would be the American route; but I prefer to leave this to the last, as the latest results relate to that route. I take next, therefore, a route which some regard as the most promising of all—that, namely, which passes between Spitzbergen and the Scandinavian peninsula.

It will be remembered that Lieutenant Payer, of the Austrian navy, had accompanied Captain Koldewey's first expedition. When driven back from the attempt to advance along the eastern shores of Greenland, that commander crossed over to Spitzbergen, and tried to find the Land of Gilles. He also accompanied Koldewey's later expedition, and shared his belief that there is no continuous channel northwards on the western side of the North Atlantic channel. Believing still, however, with Dr. Petermann, the geographer, that there is an open Polar sea beyond the ice-barrier, Payer set out in 1871, in company with Weyprecht, towards the Land of Gilles. They did not find this mysterious land, but succeeded in passing 150 miles further north, after rounding the south-eastern shores of Spitzbergen, than any Arctic voyagers who had before penetrated into the region lying between Spitzbergen and Novaia Zemlia. Here they found, beyond the 76th parallel, and between 42° and 60° east longitude, an open sea, and a temperature of between 5° and 7° above the freezing-point. Unfortunately, they had not enough provisions with them to be able safely to travel further north, and were thus compelled to return. The season seems to have been an unusually open one; and it is much to be regretted that the expedition was not better

centuries ago. Indeed, among Captain Koldewey's results is one which seems to indicate the occurrence of such a change. The country he explored was found to have been inhabited. "Numerous huts of Esquimaux were seen, and various instruments and utensils of primitive form; but for some reason or other the region seems to have been finally deserted. The Polar bear reigns supreme on the glaciers, as the walrus does among the icebergs." "Not improbably the former inhabitants were forced to leave this region by the gradually increasing cold."

supplied with provisions—a defect which appears to be not uncommon with German expeditions.

Soon after their return, Payer and Weyprecht began to prepare for a new expedition; and this time their preparations were thorough, and adapted for a long stay in Arctic regions. "The chief aim of this expedition," says the *Revue des Deux Mondes*, in an interesting account of recent Polar researches, "was to investigate the unknown regions of the Polar seas to the north of Siberia, and to try to reach Behring's Straits by this route." It was only if after two winters and three summers they failed to double the extreme promontory of Asia, that they were to direct their course towards the Pole. The voyagers, numbering twenty-four persons, left the Norwegian port of Tromsoë, in the steamer *Tegethoff*, on July 14, 1872. Count Wilczek followed shortly after in a yacht, which was to convey coals and provisions to an eastern point of the Arctic Ocean, for the benefit of the *Tegethoff*. At a point between Novaia Zemlia and the mouth of the Petschora, the yacht lost sight of the steamer, and nothing was heard of the latter for twenty-five months. General anxiety was felt for the fate of the expedition, and various efforts were made by Austria, England, and Russia to obtain news of it. In September, 1874, the voyagers suddenly turned up at another port, and soon after entered Vienna amid great enthusiasm. Their story was a strange one.

It appears that when the *Tegethoff* was lost sight of (August 21, 1872), she had been surrounded by vast masses of ice, which crushed her hull. For nearly half a year the deadly embrace of the ice continued; and when at length pressure ceased, the ship remained fixed in the ice, several miles from open water. During the whole summer the voyagers tried to release their ship, but in vain. They had not, however, remained motionless all this time. The yacht had lost sight of them at a spot between Novaia Zemlia and Malaia Zemlia (in North Russia) in about  $71^{\circ}$  north latitude, and they were imprisoned not far north of this spot. But

the ice-field was driven hither and thither by the winds, until they found themselves, on the last day of August, 1873, only 6' or about seven miles south of the 80th parallel of latitude. Only fourteen miles from them, on the north, they saw "a mass of mountainous land, with numerous glaciers." They could not reach it until the end of October, however, and then they had to house themselves in preparation for the long winter night. This land they called Francis Joseph Land. It lies north of Novaia Zemlia, and on the Polar side of the 80th parallel of latitude. The winter was stormy and bitterly cold, the thermometer descending on one occasion to  $72^{\circ}$  below zero—very nearly as low as during the greatest cold experienced by Nares's party. In February, 1874, "the sun having reappeared, Lieutenant Payer began to prepare sledge excursions to ascertain the configuration of the land. . . . In the second excursion the voyagers entered Austria Sound, which bounds Francis Joseph Island on the east and north, and found themselves, after emerging from it, in the midst of a large basin, surrounded by several large islands. The extreme northern point reached by the expedition was a cape on one of these islands, which they named Prince Rodolph's Land, calling the point Cape Fligely. It lies a little beyond the 81st parallel. They saw land further north beyond the 83rd degree of latitude, and named it Petermann's Land. The archipelago thus discovered is comparable in extent to that of which Spitzbergen is the chief island." The voyagers were compelled now to return, as the firm ice did not extend further north. They had a long, difficult, and dangerous journey southwards—sometimes on open water, in small boats, sometimes on ice, with sledges—impeded part of the time by contrary winds, and with starvation staring them in the face during the last fortnight of their journey. Fortunately, they reached Novaia Zemlia before their provisions quite failed them, and were thence conveyed to Wardhoë by a Russian trading ship.

We have now only to consider the attempts which have been made to approach the North Pole by the American

route. For, though Collinson in 1850 reached high latitudes to the north of Behring's Straits, while Wrangel and other Russian voyagers have attempted to travel northwards across the ice which bounds the northern shores of Siberia, it can hardly be said that either route has been followed with the definite purpose of reaching the North Pole. I shall presently, however, have occasion to consider the probable value of the Behring's Straits route, which about twelve years ago was advocated by the Frenchman Lambert.

Dr. Kane's expedition in 1853-55 was one of those sent out in search of Sir John Franklin. It was fitted out at the expense of the United States Government, and the route selected was that along Smith's Sound, the northerly prolongation of Baffin's Bay. Kane wintered in 1853 and 1854 in Van Reusselaer's Inlet, on the western coast of Greenland, in latitude  $78^{\circ} 43'$  north. Leaving his ship, the *Advance*, he made a boat-journey to Upernivik,  $6^{\circ}$  further south. He next traced Kennedy Channel, the northerly prolongation of Smith's Sound, reaching latitude  $81^{\circ} 22'$  north. He named heights visible yet further to the north, Parry Mountains; and at the time—that is, twenty-two years ago—the land so named was the highest northerly land yet seen. Hayes, who had accompanied Kane in this voyage, succeeded in reaching a still higher latitude in sledges drawn by Esquimaux dogs. Both Kane and Hayes agreed in announcing that where the shores of Greenland trend off eastwards from Kennedy Channel, there is an open sea, "rolling," as Captain Maury magniloquently says, "with the swell of a boundless ocean." It was in particular noticed that the tides ebbed and flowed in this sea. On this circumstance Captain Maury based his conclusion that there is an open sea to the north of Greenland. After showing that the tidal wave could not well have travelled along the narrow and icebound straits between Baffin's Bay and the region reached by Kane and Hayes, Maury says: "Those tides must have been born in that cold sea, having their cradle about the North Pole." The context shows,

however, that he really intended to signify that the waves were formed in seas around the North Pole, and thence reached the place where they were seen ; so that, as birth usually precedes cradling, Maury would more correctly have said that these tides are cradled in that cold sea, having their birth about the North Pole.

The observations of Kane and Hayes afford no reason, however, for supposing that there is open water around the North Pole. They have been rendered somewhat doubtful, be it remarked in passing, by the results of Captain Nares's expedition ; and it has been proved beyond all question that there is not an open sea directly communicating with the place where Kane and Hayes observed tidal changes. But, apart from direct evidence of this kind, two serious errors affect Maury's reasoning, as I pointed out eleven years since. In the first place, a tidal wave would be propagated quite freely along an ice-covered sea, no matter how thick the ice might be, so long as the sea was not absolutely icebound. Even if the latter condition could exist for a time, the tidal wave would burst the icy fetters that bound the sea, unless the sea were frozen to the very bottom ; which, of course, can never happen with any sea properly so called. It must be remembered that, even in the coldest winter of the coldest Polar regions, ice of only a moderate thickness can form in open sea in a single day ; but the tidal wave does not allow ice to form for a single hour in such sort as to bind the great ice-fields and the shore-ice into one mighty mass. At low tide, for a very short time, ice may form in the spaces between the shore-ice and the floating ice, and again between the various masses of floating ice, small or large (up to many square miles in extent) ; but as the tidal wave returns it breaks through these bonds as easily as the Jewish Hercules burst the withes with which the Philistines had bound his mighty limbs. It is probable that if solid ice as thick as the thickest which Nares's party found floating in the Palæo-crustic Sea—ice 200 feet thick—reached from shore to shore

of the North Atlantic channel, the tidal wave would burst the barrier as easily as a rivulet rising but a few inches bursts the thin coating which has formed over it on the first cold night of autumn. But no such massive barriers have to be broken through, for the tidal wave never gives the ice an hour's rest. Maury reasons that "the tidal wave from the Atlantic can no more pass under the icy barrier to be propagated in the seas beyond, than the vibrations of a musical string can pass with its notes a fret on which the musician has placed his finger." But the circumstances are totally different. The ice shares the motion of the tidal wave, which has not to pass under the ice, but to lift it. This, of course, it does quite as readily as though there were no ice, but only the same weight of water. The mere weight of the ice counts simply for nothing. The tidal wave would rise as easily in the British Channel if a million Great Easterns were floating there as if there was not even a cock-boat; and the weight of ice, no matter how thick or extensive, would be similarly ineffective to restrain the great wave which the sun and moon send coursing twice a day athwart our oceans. Maury's other mistake was even more important so far as this question of an open sea is concerned. "No one," as I wrote in 1867, "who is familiar with the astronomical doctrine of the tides, can believe for a moment that tides could be generated in a land-locked ocean, so limited in extent as the North Polar sea (assuming its existence) must necessarily be." To raise a tidal wave the sun and moon require not merely an ocean of wide extent to act upon, but an ocean so placed that there is a great diversity in their pull on various parts of it; for it is the difference between the pull exerted on various parts, and not the pull itself, which creates the tidal wave. Now the Polar sea has not the required extent, and is not in the proper position, for this diversity of pull to exist in sufficient degree to produce a tidal wave which could be recognized. It is certain, in fact, that, whether there is open water or not near the Pole, the tides observed by

Kane and Hayes must have come from the Atlantic, and most probably by the North Atlantic channel.

Captain Hall's expedition in the *Polaris* (really under the command of Buddington), in 1871-72, will be probably in the recollection of most of my readers. Leaving Newfoundland on June 29, 1871, it sailed up Smith's Sound, and by the end of August had reached the 80th parallel. Thence it proceeded up Kennedy Channel, and penetrated into Robeson Channel, the northerly prolongation of Kennedy Channel, and only 13 miles wide. Captain Hall followed this passage as far as  $82^{\circ} 16'$  north latitude, reaching his extreme northerly point on September 3. From it he saw "a vast expanse of open sea, which he called Lincoln Sea, and beyond that another ocean or gulf; while on the west there appeared, as far as the eye could reach, the contours of coast. This region he called Grant Land." So far as appears, there was no reason at that time why the expedition should not have gone still further north, the season apparently having been exceptionally open. But the naval commander of the expedition, Captain Buddington, does not seem to have had his heart in the work, and, to the disappointment of Hall, the *Polaris* returned to winter in Robeson Channel, a little beyond the 81st degree. In the same month, September, 1871, Captain Hall died, under circumstances which suggested to many of the crew and officers the suspicion that he had been poisoned.\* In the spring of 1870 the *Polaris* resumed her course homewards. They were greatly impeded by the ice. A party which got separated from those on board were unfortunately unable to regain the ship, and remained on an ice-field for 240 days, suffering fearfully. The ice-field, like that on which the crew of the *Hansa* had to take up their abode, drifted southwards, and was gradually diminishing, when fortunately a passing steamer observed

\* Dr. Emile Bessels was tried at New York in 1872, on the charge of having poisoned Captain Hall, but was acquitted.

the prisoners (April 30, 1872) and rescued them. The *Polaris* herself was so injured by the ice that her crew had to leave her, wintering on Lyttelton Island. They left this spot in the early summer of 1872, in two boats, and were eventually picked up by a Scotch whaler.

Captain Nares's expedition followed Hall's route. I do not propose to enter here into any of the details of the voyage, with which my readers are no doubt familiar. The general history of the expedition must be sketched, however, in order to bring it duly into its place here. The *Alert* and *Discovery* sailed under Captains Nares and Stephenson, in May, 1875. Their struggle with the ice did not fairly commence until they were nearing the 79th parallel, where Baffin's Bay merges into Smith's Sound. Thence, through Smith's Sound, Kennedy Channel, and Robeson Channel, they had a constant and sometimes almost desperate struggle with the ice, until they had reached the north end of Robeson Channel. Here the *Discovery* took up her winter quarters, in north latitude  $81^{\circ} 44'$ , a few miles north of Captain Hall's wintering-place, but on the opposite (or westerly) side of Robeson Channel. The *Alert* still struggled northwards, rounding the north-east point of Grant Land, and there finding, not, as was expected, a continuous coast-line on the west, but a vast ice-bound sea. No harbour could be found, and the ship was secured on the inside of a barrier of grounded ice, in latitude  $82^{\circ} 31'$ , in the most northerly wintering-place ever yet occupied by man. The ice met with on this sea is described as "of most unusual age and thickness, resembling in a marked degree, both in appearance and formation, low floating icebergs rather than ordinary salt-water ice. Whereas ordinary ice is from 2 feet to 10 feet in thickness, that in this Polar sea has gradually increased in age and thickness until it measures from 80 feet to 120 feet, floating with its surface at the lower part 15 feet above the water-line. In some places the ice reaches a thickness of from 150 to 200 feet, and the general impression among

the officers of the expedition seems to have been that the ice of this Palæocystic Sea is the accumulation of many years, if not of centuries ; "that the sea is never free of it and never open ; and that progress to the Pole through it or over it is impossible with our present resources."

The winter which followed was the bitterest ever known by man. For 142 days the sun was not seen ; the mercury was frozen during nearly nine weeks. On one occasion the thermometer showed 104° below the freezing-point, and during one terrible fortnight the mean temperature was 91° below freezing !

As soon as the sun reappeared sledge-exploration began, each ship being left with only half-a-dozen men and officers on board. Expeditions were sent east and west, one to explore the northern coast of Greenland, the other to explore the coast of Grant Land. Captain Stephenson crossed over from the *Discovery's* wintering-place to Polaris Bay, and there placed over Hall's grave a tablet, prepared in England, bearing the following inscription : "Sacred to the memory of Captain C. F. Hall, of U.S. *Polaris*, who sacrificed his life in the advancement of science, on November 8, 1871. This tablet has been erected by the British Polar Expedition of 1875, who, following in his footsteps, have profited by his experience"—a graceful acknowledgment (which might, however, have been better expressed). The party which travelled westwards traced the shores of Grant Land as far as west longitude 86° 30', the most northerly cape being in latitude 83° 7', and longitude 70° 30' west. This cape they named Cape Columbia.

The coast of Greenland was explored as far east as longitude 50° 40' (west), land being seen as far as 82° 54' north, longitude 48° 33' west. Lastly, a party under Commander Markham and Lieutenant Parr pushed northwards. They were absent ten weeks, but had not travelled so far north in the time as was expected, having encountered great difficulties. On May 12, 1876, they reached their most northerly point, planting the British flag in latitude

83° 20' 26" north. "Owing to the extraordinary nature of the pressed-up ice, a roadway had to be formed by pickaxes for nearly half the distance travelled, before any advance could be safely made, even with light loads; this rendered it always necessary to drag the sledge-loads forward by instalments, and therefore to journey over the same road several times. The advance was consequently very slow, and only averaged about a mile and a quarter daily—much the same rate as was attained by Sir Edward Parry during the summer of 1827. The greatest journey made in any one day amounted only to two miles and three quarters. Although the distance made good was only 73 miles from the ship, 276 miles were travelled over to accomplish it." It is justly remarked, in the narrative from which I have made this extract, that no body of men could have surpassed in praiseworthy perseverance this gallant party, whose arduous struggle over the roughest and most monotonous road imaginable, may fairly be regarded as surpassing all former exploits of the kind. (The narrator says that it has "eclipsed" all former ones, which can scarcely be intended to be taken *au pied de la lettre*.) The expedition reached the highest latitude ever yet attained under any conditions, carried a ship to higher latitudes than any ship had before reached, and wintered in higher latitudes than had ever before been dwelt in during the darkness of a Polar winter. They explored the most northerly coast-line yet traversed, and this both on the east and west of their route northwards. They have ascertained the limits of human habitation upon this earth, and have even passed beyond the regions which animals occupy, though nearly to the most northerly limit of the voyage they found signs of the occasional visits of warm-blooded animals. Last, but not least, they have demonstrated, as it appears to me (though possibly Americans will adopt a different opinion), that by whatever route the Pole is to be reached, it is not by that which I have here called the American route, at least with the present means of transit over icebound seas. The

country may well be satisfied with such results (apart altogether from the scientific observations, which are the best fruits of the expedition), even though the Pole has not yet been reached.

Must we conclude, however, that the North Pole is really inaccessible? It appears to me that the annals of Arctic research justify no such conclusion. The attempt which has just been made, although supposed at the outset to have been directed along the most promising of all the routes heretofore tried, turned out to be one of the most difficult and dangerous. Had there been land extending northwards (as Sherard Osborn and others opined), on the western side of the sea into which Robeson Channel opens, a successful advance might have been made along its shore by sledging. M'Clintock, in 1853, travelled 1220 miles in 105 days; Richards 1012 miles in 102 days; Mecham 1203 miles; Richards and Osborn 1093 miles; Hamilton 1150 miles with a dog-sledge and one man. In 1854 Mecham travelled 1157 miles in only 70 days; Young travelled 1150 miles and M'Clintock 1330 miles. But these journeys were made either over land or over unmoving ice close to a shore-line. Over an icebound sea journeys of the kind are quite impracticable. But the conditions, while not more favourable in respect of the existence of land, were in other respects altogether less favourable along the American route than along any of the others I have considered in this brief sketch of the attempts hitherto made to reach the Pole.

The recent expedition wintered as near as possible to the region of maximum winter cold in the western hemisphere, and pushed their journey northwards athwart the region of maximum summer cold. Along the course pursued by Parry's route the cold is far less intense, in corresponding latitudes, than along the American route; and cold is the real enemy which bars the way towards the Pole. All the difficulties and dangers of the journey either have their origin (as directly as the ice itself) in the bitter Arctic cold, or are rendered effective and intensified by the cold. The

course to be pursued, therefore, is that indicated by the temperature. Where the July isotherms, or lines of equal summer heat, run northwards, a weak place is indicated in the Arctic barrier ; where they trend southwards, that barrier is strongest. Now there are two longitudes in which the July Arctic isotherms run far northward of their average latitude. One passes through the Parry Islands, and indicates the sea north-east of Behring's Straits as a suitable region for attack ; the other passes through Spitzbergen, and indicates the course along which Sir E. Parry's attack was made. The latter is slightly the more promising line of the two, so far as temperature is concerned, the isotherm of  $36^{\circ}$  Fahrenheit (in July) running here as far north as the 77th parallel, whereas its highest northerly range in the longitude of the Parry Islands is but about  $76^{\circ}$ . The difference, however, is neither great nor altogether certain ; and the fact that Parry found the ice drifting southwards, suggests the possibility that that *may* be the usual course of oceanic currents in that region. North of the Parry Islands the drift may be northwardly, like that which Payer and Weyprecht experienced to the north of Novaia Zemlia.

There is one great attraction for men of science in the route by the Parry Islands. The magnetic pole has almost certainly travelled into that region. Sir J. Ross found it, indeed, to be near Boothia Gulf, far to the east of the Parry Islands, in 1837. But the variations of the needle all over the world since then, indicate unmistakably that the magnetic poles have been travelling round towards the west, and at such a rate that the northern magnetic pole has probably nearly reached by this time the longitude of Behring's Straits. The determination of the exact present position of the Pole would be a much more important achievement, so far as science is concerned, than a voyage to the pole of rotation.

There is one point which suggests itself very forcibly in reading the account of the sledgeging expedition from the *Alert* towards the north. In his official report, Captain

Nares says that "half of each day was spent in dragging the sledges in that painful fashion—face toward the boat—in which the sailors drag a boat from the sea on to the sand ;" and again he speaks of the "toilsome dragging of the sledges over ice-ridges which resembled a stormy sea suddenly frozen." In doing this "276 miles were toiled over in travelling only 73 miles." Is it altogether clear that the sledges were worth the trouble ? One usually regards a sledge as intended to carry travellers and their provisions, etc., over ice and snow, and as useful when so employed ; but when the travellers have to take along the sledge, going four times as far and working ten times as hard as if they were without it, the question suggests itself whether all necessary shelter, provisions, and utensils might not have been much more readily conveyed by using a much smaller and lighter sledge, and by distributing a large part of the luggage among the members of the expedition. The parts of a small hut could, with a little ingenuity, be so constructed as to admit of being used as levers, crowbars, carrying-poles, and so forth, and a large portion of the luggage absolutely necessary for the expedition could be carried by their help ; while a small, light sledge for the rest could be helped along and occasionally lifted bodily over obstructions by levers and beams forming part of the very material which by the usual arrangement forms part of the load. I am not suggesting, be it noticed, that by any devices of this sort a journey over the rough ice of Arctic regions could be made easy. But it does seem to me that if a party could go back and forth over 276 miles, pickaxing a way for a sledge, and eventually dragging it along over the path thus pioneered for it, and making only an average of  $1\frac{1}{4}$  mile of real progress per day, or 73 miles in all, the same men could with less labour (though still, doubtless, with great toil and trouble) make six or seven miles a day by reducing their *impedimenta* to what could be carried directly along with them. Whether use might not be made of the lifting power of buoyant gas, is a question

which only experienced aéronauts and Arctic voyagers could answer. I believe that the employment of imprisoned balloon-power for many purposes, especially in time of war, has received as yet much less attention than it deserves. Of course I am aware that in Arctic regions many difficulties would present themselves ; and the idea of ordinary ballooning over the Arctic ice-fields may be regarded as altogether wild in the present condition of the science of aéronautics. But the use of balloon-power as an auxiliary, however impracticable at present, is by no means to be despised of as science advances.

After all, however, the advance upon the Pole itself, however interesting to the general public, is far less important to science than other objects which Arctic travellers have had in view. The inquiry into the phenomena of terrestrial magnetism within the Arctic regions ; the investigation of oceanic movements there ; of the laws according to which low temperatures are related to latitude and geographical conditions ; the study of aërial phenomena ; of the limits of plant life and animal life ; the examination of the mysterious phenomena of the Aurora Borealis—these and many other interesting subjects of investigation have been as yet but incompletely dealt with. In the Polar regions, as Maury well remarked, “the icebergs are framed and glaciers launched ; there the tides have their cradle, the whales their nursery ; there the winds complete their circuit, and the currents of the sea their round, in the wonderful system of oceanic circulation ; there the Aurora is lighted up, and the trembling needle brought to rest ; and there, too, in the mazes of that mystic circle, terrestrial forces of occult power and of vast influence upon the well-being of man are continually at work. It is a circle of mysteries ; and the desire to enter it, to explore its untrodden wastes and secret chambers, and to study its physical aspects, has grown into a longing. Noble daring has made Arctic ice and snow-clad seas classic ground.”

## *A MIGHTY SEA-WAVE.*

ON May 10th, 1876, a tremendous wave swept the Pacific Ocean from Peru northwards, westwards, and southwards, travelling at a rate many times greater than that of the swiftest express train. For reasons best known to themselves, writers in the newspapers have by almost common consent called this phenomenon a tidal-wave. But the tides had nothing to do with it. Unquestionably the wave resulted from the upheaval of the bed of the ocean in some part of that angle of the Pacific Ocean which is bounded by the shores of Peru and Chili. This region has long been celebrated for tremendous submarine and subterranean upheavals. The opinions of geologists and geographers have been divided as to the real origin of the disturbances by which at one time the land, at another time the sea, and at yet other times (oftener, in fact, than either of the others) both land and sea have been shaken as by some mighty imprisoned giant, struggling, like Prometheus, to cast from his limbs the mountain masses which hold them down. Some consider that the seat of the Vulcanian forces lies deep below that part of the chain of the Andes which lies at the apex of the angle just mentioned, and that the direction of their action varies according to the varying conditions under which the imprisoned gases find vent. Others consider that there are two if not several seats of subterranean activity. Yet others suppose that the real seat of disturbance lies beneath

the ocean itself, a view which seems to find support in several phenomena of recent Peruvian earthquakes.

Although we have not full information concerning the great wave which in May, 1876, swept across the Pacific, and northwards and southwards along the shores of the two Americas, it may be interesting to consider some of the more striking features of this great disturbance of the so-called peaceful ocean, and to compare them with those which have characterized former disturbances of a similar kind. We may thus, perhaps, find some evidence by which an opinion may be formed as to the real seat of subterranean activity in this region.

At the outset it may be necessary to explain why I have asserted somewhat confidently that the tides have nothing whatever to do with this great oceanic wave. It is of course well known to every reader that the highest or spring tides occur always two or three days after new moon and after full moon, the lowest (or rather the tides having least range above and below the mean level) occurring two or three days after the first and third quarters of the lunar month. The sun and moon combine, indeed, to sway the ocean most strongly at full and new, while they pull contrariwise at the first and third quarters; but the full effect of their combined effort is only felt a few days later than when it is made, while the full effect of their opposition to each other, in diminishing the range of the oceanic oscillation, is also only felt after two or three days. Thus in May, 1876, the tidal wave had its greatest range on or about May 16, new moon occurring at half-past five on the morning of May 13; and the tidal wave had its least range on or about May 8, the moon passing her third quarter a little after eleven on the morning of May 4. Accordingly the disturbance which affected the waters of the Pacific so mightily on May 10, occurred two days after the lowest or neap tides, and five days before the highest or spring tides. Manifestly that was not a time when a tidal wave of exceptional height could be expected,

or, indeed, could possibly occur. Such a wave as actually disturbed the Pacific on that day could not in any case have been produced by tidal action, even though the winds had assisted to their utmost, and all the circumstances which help to make high tides had combined—as the greatest proximity of moon to earth, the conjunction of moon and sun near the celestial equator, and (of course) the exact coincidence of the time of the tidal disturbance with that when the combined pull of the sun and moon is strongest. As, instead, the sun was nearly eighteen degrees from the equator, the moon more than nine, and as the moon was a full week's motion from the part of her path where she is nearest to the earth, while, as we have seen, only two days had passed from the time of absolutely lowest tides, it will be seen how utterly unable the tidal-wave must have been on the day of the great disturbance to produce the effects presently to be described.

It may seem strange, in dealing with the case of a wave which apparently had its origin in or near Peru on May 9, to consider the behaviour of a volcano, distant 5000 miles from this region, a week before the disturbance took place. But although the coincidence may possibly have been accidental, yet in endeavouring to ascertain the true seat of disturbance we must overlook no evidence, however seemingly remote, which may throw light on that point; and as the sea-wave generated by the disturbance reached very quickly the distant region referred to, it is by no means unlikely that the subterranean excitement which the disturbance relieved may have manifested its effects beforehand at the same remote volcanic region. Be this as it may, it is certain that on May 1 the great crater of Kilauea, in the island of Hawaii, became active, and on the 4th severe shocks of earthquake were felt at the Volcano House. At three in the afternoon a jet of lava was thrown up to a height of about 100 feet, and afterwards some fifty jets came into action. Subsequently jets of steam issued along the line formed by a fissure four miles in length

down the mountain-side. The disturbance lessened considerably on the 5th, and an observing party examined the crater. They found that a rounded hill, 700 feet in height, and 1400 feet in diameter, had been thrown up on the plain which forms the floor of the crater. Fire and scoria spouted up in various places.

Before rejecting utterly the belief that the activity thus exhibited in the Hawaii volcano had its origin in the same subterrene or submarine region as the Peruvian earthquake, we should remember that other regions scarcely less remote have been regarded as forming part of the same Vulcanian district. The violent earthquakes which occurred at New Madrid, in Missouri, in 1812, took place at the same time as the earthquake of Caraccas, the West Indian volcanoes being simultaneously active; and earthquakes had been felt in South Carolina for several months before the destruction of Caraccas and La Guayra. Now we have abundant evidence to show that the West Indian volcanoes are connected with the Peruvian and Chilian regions of Vulcanian energy, and the Chilian region is about as far from New Madrid as Arica in Peru from the Sandwich Isles.

It was not, however, until about half-past eight on the evening of May 9 that the Peruvian earthquake began. A severe shock, lasting from four to five minutes, was felt along the entire southern coast, even reaching Antofagasta. The shock was so severe that it was impossible, in many places, to stand upright. It was succeeded by several others of less intensity.

While the land was thus disturbed, the sea was observed to be gradually receding, a movement which former experiences have taught the Peruvians to regard with even more terror than the disturbance of the earth itself. The waters which had thus withdrawn, as if concentrating their energies to leap more fiercely on their prey, presently returned in a mighty wave, which swept past Callao, travelling southwards with fearful velocity, while in its train followed wave after wave, until no less than eight had taken

their part in the work of destruction. At Mollendo the railway was torn up by the sea for a distance of 300 feet. A violent hurricane which set in afterwards from the south prevented all vessels from approaching, and unroofed most of the houses in the town. At Arica the people were busily engaged in preparing temporary fortifications to repel a threatened assault of the rebel ram *Huiscar*, at the moment when the roar of the earthquake was heard. The shocks here were very numerous, and caused immense damage in the town, the people flying to the Morro for safety. The sea was suddenly perceived to recede from the beach, and a wave from ten feet to fifteen feet in height rolled in upon the shore, carrying before it all that it met. Eight times was this assault of the ocean repeated. The earthquake had levelled to the ground a portion of the custom-house, the railway station, the submarine cable office, the hotel, the British Consulate, the steamship agency, and many private dwellings. Owing to the early hour of the evening, and the excitement attendant on the proposed attack of the *Huiscar*, every one was out and stirring; but the only loss of life which was reported was that of three little children who were overtaken by the water. The progress of the wave was only stopped at the foot of the hill on which the church stands, which point is further inland than that reached in August, 1868. Four miles of the embankment of the railway were swept away like sand before the wind. Locomotives, cars, and rails, were hurled about by the sea like so many playthings, and left in a tumbled mass of rubbish.

The account proceeds to say that the United States steamer *Waters*, stranded by the bore of 1868, was lifted up bodily by the wave at Arica, and floated two miles north of her former position. The reference is no doubt to the double-ender *Watertree*, not stranded by a bore (a term utterly inapplicable to any kind of sea-wave at Arica, where there is no large river), but carried in by the great wave which followed the earthquake of August 13. The description of the wave at Arica on that occasion should be

compared with that of the wave of May, 1876. About twenty minutes after the first earth shock, the sea was seen to retire, as if about to leave the shores wholly dry; but presently its waters returned with tremendous force. A mighty wave, whose length seemed immeasurable, was seen advancing like a dark wall upon the unfortunate town, a large part of which was overwhelmed by it. Two ships, the Peruvian corvette *America*, and the American double-ender *Watertree*, were carried nearly half a mile to the north of Arica, beyond the railroad which runs to Tacna, and there left stranded high and dry. As the English vice-consul at Arica estimated the height of this enormous wave at fully fifty feet, it would not seem that the account of the wave of May, 1876, has been exaggerated, for a much less height is, as we have seen, attributed to it, though, as it carried the *Watertree* still further inland, it must have been higher. The small loss of life can be easily understood when we consider that the earthquake was not followed instantly by the sea-wave. Warned by the experience of the earthquake of 1868, which most of them must have remembered, the inhabitants sought safety on the higher grounds until the great wave and its successors had flowed in. We read that the damage done was greater than that caused by the previous calamity, the new buildings erected since 1868 being of a more costly and substantial class. Merchandise from the custom-house and stores was carried by the water to a point on the beach five miles distant.

At Iquique, in 1868, the great wave was estimated at fifty feet in height. We are told that it was black with the mud and slime of the sea bottom. "Those who witnessed its progress from the upper balconies of their houses, and presently saw its black mass rushing close beneath their feet, looked on their safety as a miracle. Many buildings were, indeed, washed away, and in the low-lying parts of the town there was a terrible loss of life." In May, 1876, the greatest mischief at Iquique would seem to have been caused by the earthquake, not by the sea-wave, though

this also was destructive in its own way. "Iquique," we are told, "is in ruins. The movement was experienced there at the same time and with the same force [as at Arica]. Its duration was exactly four minutes and a third. It proceeded from the south-east, exactly from the direction of Ilaga." The houses built of wood and cane tumbled down at the first attacks, lamps were broken, and the burning oil spread over and set fire to the ruins. Three companies of firemen, German, Italian, and Peruvian, were instantly at their posts, although it was difficult to maintain an upright position, shock following shock with dreadful rapidity. Nearly 400,000 quintals of nitrate in the stores at Iquique and the adjacent ports of Molle and Pisagua were destroyed. The British barque *Caprera* and a German barque sank, and all the coasting craft and small boats in the harbour were broken to pieces and drifted about in every direction.

At Chanavaya, a small town at the guano-loading dépôt known as Pabellon de Pica, only two houses were left standing out of four hundred. Here the earthquake shock was specially severe. In some places the earth opened in crevices seventeen yards deep and the whole surface of the ground was changed.

At Punta de Eobos two vessels were lost, and fourteen ships more or less damaged, by the wave. Antofagasta, Mexillones, Tocopilla, and Cobigo, on the Bolivian coast, suffered simultaneously from the earthquake and the sea-wave. The sea completely swept the business portion of Antofagasta during four hours. Here a singular phenomenon was noticed. For some time the atmosphere was illuminated with a ruddy glow. It was supposed that this light came from the volcano of San Pedro de Atacama, a few leagues inland from Antofagasta. A somewhat similar phenomenon was noticed at Tacna during the earthquake of August, 1868. About three hours after the earthquake an intensely brilliant light made its appearance above the neighbouring mountains. It lasted fully half an hour, and was ascribed to the eruption of some as yet unknown volcano.

As to the height of the great wave along this part of the shore-line of South America, the accounts vary. According to those which are best authenticated, it would seem as though the wave exceeded considerably in height that which flowed along the Peruvian, Bolivian, and Chilian shores in August, 1868. At Huaniles the wave was estimated at sixty feet in height, at Mexillones, where the wave, as it passed southwards, ran into Mexillones Bay, it reached a height of sixty-five feet. Two-thirds of the town were completely obliterated, wharves, railway stations, distilleries, etc., all swallowed up by the sea.

The shipping along the Peruvian and Bolivian coast suffered terribly. The list of vessels lost or badly injured at Pabellon de Pica alone, reads like the list of a fleet.

I have been particular in thus describing the effects produced by the earthquake and sea-wave on the shores of South America, in order that the reader may recognize in the disturbance produced there the real origin of the great wave which a few hours later reached the Sandwich Isles, 5000 miles away. Doubt has been entertained respecting the possibility of a wave, other than the tidal-wave, being transmitted right across the Pacific. Although in August, 1868, the course of the great wave which swept from some region near Peru, not only across the Pacific, but in all directions over the entire ocean, could be clearly traced, there were some who considered the connection between the oceanic phenomena and the Peruvian earthquake a mere coincidence. It is on this account perhaps chiefly that the evidence obtained in May, 1876, is most important. It is interesting, indeed, as showing how tremendous was the disturbance which the earth's frame must then have undergone. It would have been possible, however, had we no other evidence, for some to have maintained that the wave which came in upon the shores of the Sandwich Isles a few hours after the earthquake and sea disturbance in South America was in reality an entirely independent phenomenon. But when we compare the events which happened in May, 1876,

with those of August, 1868, and perceive their exact similarity, we can no longer reasonably entertain any doubt of the really stupendous fact that *the throes of the earth in and near Peru are of sufficient energy to send oceanic waves right across the Pacific*,—waves, too, of such enormous height at starting, that, after travelling with necessarily diminishing height the whole way to Hawaii, they still rose and fell through thirty-six feet. The real significance of this amazing oceanic disturbance is exemplified by the wave circles which spread around the spot where a stone has fallen into a smooth lake. We know how, as the circles widen, the height of the wave grows less and less, until, at no great distance from the centre of disturbance, the wave can no longer be discerned, so slight is the slope of its advancing and following faces. How tremendous, then, must have been the upheaval of the bed of ocean by which wave-circles were sent across the Pacific, retaining, after travelling 5000 miles from the centre of disturbance, the height of a two-storied house! In 1868, indeed, we know that the wave travelled very much further, reaching the shores of Japan, of New Zealand, and of Australia, even if it did not make its way through the East Indian Archipelago to the Indian Ocean, as some observations seem to show. Although no news has been received which would justify us in believing that the wave of May, 1876, produced corresponding effects at such great distances from the centre of disturbance, it must be remembered that the dimensions of the wave when it reached the Sandwich Isles fell far short of those of the great wave of August 13-14, 1868.

It will be well to make a direct comparison between the waves of May, 1876, and August, 1868, in this respect, as also with regard to the rate at which they would seem to have traversed the distance between Peru and Hawaii. On this last point, however, it must be noted that we cannot form an exact opinion until we have ascertained the real region of Vulcanian disturbance on each occasion. It is possible that a careful comparison of times, and of the direction in which the wave front advanced upon different shores, might serve

to show where this region lay. I should not be greatly surprised to learn that it was far from the continent of South America.

The great wave reached the Sandwich Isles between four and five on the morning of May 10, corresponding to about five hours later of Peruvian time. An oscillation only was first observed at Hilo, on the east coast of the great southern island of Hawaii, the wave itself not reaching the village till about a quarter before five. The greatest difference between the crest and trough of the wave was found to be thirty-six feet here; but at the opposite side of the island, in Kealakekua Bay (where Captain Cook was killed), amounted only to thirty feet. In other places the difference was much less, being in some only three feet, a circumstance doubtless due to interference, waves which have reached the same spot along different courses chancing so to arrive that the crest of one corresponded with the trough of the other, so that the resulting wave was only the difference of the two. We must explain, however, in the same way, the highest waves of thirty-six to forty feet, which were doubtless due to similar interference, crest agreeing with crest and trough with trough, so that the resulting wave was the sum of the two which had been divided, and had reached the same spot along different courses. It would follow that the higher of the two waves was about twenty-one feet high, the lower about eighteen feet high; but as some height would be lost in the encounter with the shore-line, wherever it lay, on which the waves divided, we may fairly assume that in the open ocean, before reaching the Sandwich group, the wave had a height of nearly thirty feet from trough to crest. We read, in accordance with this explanation, that "the regurgitations of the sea were violent and complex, and continued through the day."

The wave, regarded as a whole, seems to have reached all the islands at the same time. Since this has not been contradicted by later accounts, we are compelled to conclude that the wave reached the group with its front parallel to the

length of the group, so that it must have come (arriving as it did from the side towards which Hilo lies) from the north-east. It was, then, not the direct wave from Peru, but the wave reflected from the shores of California, which produced the most marked effects. We can understand well, this being so, that the regurgitations of the sea were complex. Any one who has watched the inflow of waves on a beach so lying within an angle of the line that while one set of waves comes straight in from the sea, another thwart set comes from the shore forming the other side of the angle, will understand how such waves differ from a set of ordinary rollers. The crests of the two sets form a sort of network, ever changing as each set rolls on ; and considering any one of the four-cornered meshes of this wave-net, the observer will notice that while the middle of the raised sides rises little above the surrounding level, because here the crests of one set cross the troughs of the other, the corners of each quadrangle are higher than they would be in either set taken separately, while the middle of the four-cornered space is correspondingly depressed. The reason is that at the corners of the wave-net crests join with crests to raise the water surface, while in the middle of the net (not the middle of the sides, but the middle of the space enclosed by the four sides) trough joins with trough to depress the water surface.\*

We must take into account the circumstance that the wave which reached Hawaii in May, 1876, was probably reflected from the Californian coast, when we endeavour to determine the rate at which the sea disturbance was propagated across

\* The phenomena here described are well worth observing on their own account, as affording a very instructive and at the same time very beautiful illustration of wave motions. They can be well seen at many of our watering-places. The same laws of wave motion can be readily illustrated also by throwing two stones into a large smooth pool, at points a few yards apart. The crossing of the two sets of circular waves produces a wave-net, the meshes of which vary in shape according to their position.

the Atlantic. The direct wave would have come sooner, and may have escaped notice because arriving in the night-time, as it would necessarily have done if a wave which travelled to California, and thence, after reflection, to the Sandwich group arrived there at a quarter before five in the morning following the Peruvian earthquake. We shall be better able to form an opinion on this point after considering what happened in August, 1868.

The earth-throe on that occasion was felt in Peru about five minutes past five on the evening of August 13. Twelve hours later, or shortly before midnight, August 13, Sandwich Island time (corresponding to 5 p.m., August 14, Peruvian time), the sea round the group of the Sandwich Isles rose in a surprising manner, "insomuch that many thought the islands were sinking, and would shortly subside altogether beneath the waves. Some of the smaller islands were for a time completely submerged. Before long, however, the sea fell again, and as it did so the observers found it impossible to resist the impression that the islands were rising bodily out of the water. For no less than three days this strange oscillation of the sea continued to be experienced, the most remarkable ebbs and floods being noticed at Honolulu, on the island of Woahoo."

The distance between Honolulu and Arica is about 6300 statute miles; so that, if the wave travelled directly from the shores of Peru to the Sandwich Isles, it must have advanced at an average rate of about 525 miles an hour (about 450 knots an hour). This is nearly half the rate at which the earth's surface near the equator is carried round by the earth's rotation, or is about the rate at which parts in latitude 62 or 63 degrees north are carried round by rotation; so that the motion of the great wave in 1868 was fairly comparable with one of the movements which we are accustomed to regard as cosmical. I shall presently have something more to say on this point.

Now in May, 1876, as we have seen, the wave reached Hawaii at about a quarter to five in the morning, correspond-

ing to about ten, Peruvian time. Since, then, the earthquake was felt in Peru at half-past eight on the previous evening, it follows that the wave, if it travelled directly from Peru, must have taken about  $13\frac{1}{2}$  hours—or an hour and a half longer, in travelling from Peru to the Sandwich Isles, than it took in August, 1868. This is unlikely, because ocean-waves travel nearly at the same rate in the same parts of the ocean, whatever their dimensions, so only that they are large. We have, then, in the difference of time occupied by the wave in May, 1876, and in August, 1868, in reaching Hawaii, some confirmation of the result to which we were led by the arrival of the wave simultaneously at all the islands of the Sandwich group—the inference, namely, that the observed wave had reached these islands after reflection from the Californian shore-line. As the hour when the direct wave probably reached Hawaii was about a quarter past three in the morning, when not only was it night-time but also a time when few would be awake to notice the rise and fall of the sea, it seems not at all improbable that the direct wave escaped notice, and that the wave actually observed was the reflected wave from California. The direction, also, in which the oscillation was first observed corresponds well with this explanation.

It is clear that the wave which traversed the Pacific in May, 1876, was somewhat inferior in size to that of August, 1868, which therefore still deserves to be called (as I then called it) the greatest sea-wave ever known. The earthquake, indeed, which preceded the oceanic disturbance of 1868 was far more destructive than that of May, 1876, and the waves which came in upon the Peruvian and Bolivian shores were larger. Nevertheless, the wave of May, 1876, was not so far inferior to that of August, 1868, but that its course could be traced athwart the entire extent of the Pacific Ocean.

When we consider the characteristic features of the Peruvian and Chilian earthquakes, and especially when we note how wide is the extent of the region over which their

action is felt in one way or another, it can scarcely be doubted that the earth's Vulcanian energies are at present more actively at work throughout that region than in any other. There is nothing so remarkable, one may even say so stupendous, in the history of subterranean disturbance as the alternation of mighty earth-throes by which, at one time, the whole of the Chilian Andes seem disturbed and anon the whole of the Peruvian Andes. In Chili scarcely a year ever passes without earthquakes, and the same may be said of Peru; but so far as great earthquakes are concerned the activity of the Peruvian region seems to synchronize with the comparative quiescence of the Chilian region, and *vice versa*. Thus, in 1797, the terrible earthquake occurred which is known as the earthquake of Riobamba, which affected the entire Peruvian earthquake region. Thirty years later a series of tremendous throes shook the whole of Chili, permanently elevating its long line of coast to the height of several feet. During the last twelve years the Peruvian region has in turn been disturbed by great earthquakes. It should be added that between Chili and Peru there is a region about five hundred miles in length in which scarcely any volcanic action has been observed. And singularly enough, "this very portion of the Andes, to which one would imagine that the Peruvians and Chilians would fly as to a region of safety, is the part most thinly inhabited; insomuch that, as Von Buch observes, it is in some places entirely deserted."

One can readily understand that this enormous double region of earthquakes, whose oscillations on either side of the central region of comparative rest may be compared to the swaying of a mighty see-saw on either side of its point of support, should be capable of giving birth to throes propelling sea-waves across the Pacific Ocean. The throe actually experienced at any given place is relatively but an insignificant phenomenon: it is the disturbance of the entire region over which the throe is felt which must be considered in attempting to estimate the energy of the disturbing cause. The region shaken by the earthquake of 1868, for instance,

was equal to at least a fourth of Europe, and probably to fully one-half. From Quito southwards as far as Iquique—or along a full third part of the length of the South American Andes—the shock produced destructive effects. It was also distinctly felt far to the north of Quito, far to the south of Iquique, and inland to enormous distances. The disturbing forces which thus shook 1,000,000 square miles of the earth's surface must have been of almost inconceivable energy. If directed entirely to the upheaval of a land region no larger than England, those forces would have sufficed to have destroyed utterly every city, town, and village within such a region; if directed entirely to the upheaval of an oceanic region, they would have been capable of raising a wave which would have been felt on every shore-line of the whole earth. Divided even between the ocean on the one side and a land region larger than Russia in Europe on the other, those Vulcanian forces shook the whole of the land region, and sent athwart the largest of our earth's oceans a wave which ran in upon shores 10,000 miles from the centre of disturbance with a crest thirty feet high. Forces such as these may fairly be regarded as cosmical; they show unmistakably that the earth has by no means settled down into that condition of repose in which some geologists still believe. We may ask with the late Sir Charles Lyell whether, after contemplating the tremendous energy thus displayed by the earth, any geologist will continue to assert that the changes of relative level of land and sea, so common in former ages of the world, have now ceased? and agree with him that if, in the face of such evidence, a geologist persists in maintaining this favourite dogma, it would be vain to hope, by accumulating proofs of similar convulsions during a series of ages, to shake the tenacity of his conviction—

“*Si fractus illabatur orbis,  
Impavidum ferient ruinæ.*”

But there is one aspect in which such mighty sea-waves as, in 1868 and again in May, 1876, have swept over the surface of our terrestrial oceans, remains yet to be considered.

The oceans and continents of our earth must be clearly discernible from her nearer neighbours among the planets—from Venus and Mercury on the inner side of her path around the sun, and from Mars (though under less favourable conditions) from the outer side. When we consider, indeed, that the lands and seas of Mars can be clearly discerned with telescopic aid from our earth at a distance of forty millions of miles, we perceive that our earth, seen from Venus at little more than half this distance, must present a very interesting appearance. Enlarged, owing to greater proximity, nearly fourfold, having a diameter nearly twice as great as that of Mars, so that at the same distance her disc would seem more than three times as large, more brightly illuminated by the sun in the proportion of about five to two, she would shine with a lustre exceeding that of Mars, when in full brightness in the midnight sky, about thirty times ; and all her features would of course be seen with correspondingly increased distinctness. Moreover, the oceans of our earth are so much larger in relative extent than those of Mars, covering nearly three-fourths instead of barely one-half of the surface of the world they belong to, that they would appear as far more marked and characteristic features than the seas and lakes of Mars. When the Pacific Ocean, indeed, occupies centrally the disc of the earth which at the moment is turned towards any planet, nearly the whole of that disc must appear to be covered by the ocean. Under such circumstances the passage of a wide-spreading series of waves over the Pacific, at the rate of about 500 miles an hour, is a phenomenon which could scarcely fail to be discernible from Venus or Mercury, if either planet chanced to be favourably placed for the observation of the earth—always supposing there were observers in Mercury or Venus, and that these observers were provided with powerful telescopes.

It must be remembered that the waves which spread over the Pacific on August 13-14, 1868, and again on May 9-10, 1876, were not only of enormous range in length (measured along crest or trough), but also of enormous

breadth (measured from crest to crest, or from trough to trough). Were it otherwise, indeed, the progress of a wave forty or fifty feet high (at starting, and thirty-five feet high after travelling 6000 miles), at the rate of 500 miles per hour, must have proved destructive to ships in the open ocean as well as along the shore-line. Suppose, for instance, the breadth of the wave from crest to crest one mile, then, in passing under a ship at the rate of 500 miles per hour, the wave would raise the ship from trough to crest—that is, through a height of forty feet—in one-thousandth part of an hour (for the distance from trough to crest is but half the breadth of the wave), or in less than four seconds, lowering it again in the same short interval of time, lifting and lowering it at the same rate several successive times. The velocity with which the ship would travel upwards and downwards would be greatest when she was midway in her ascent and descent, and would then be equal to about the velocity with which a body strikes the ground after falling from a height of four yards. It is hardly necessary to say that small vessels subjected to such tossing as this would inevitably be swamped. On even the largest ships the effect of such motion would be most unpleasantly obvious. Now, as a matter of fact, the passage of the great sea-wave in 1868 was not noticed at all on board ships in open sea. Even within sight of the shore of Peru, where the oscillation of the sea was most marked, the motion was such that its effects were referred to the shore. We are told that observers on the deck of a United States' war steamer distinctly saw the "peaks of the mountains in the chain of the Cordilleras wave to and fro like reeds in a storm;" the fact really being that the deck on which they stood was swayed to and fro. This, too, was in a part of the sea where the great wave had not attained its open sea form, but was a rolling wave, because of the shallowness of the water. In the open sea, we read that the passage of the great sea-wave was no more noticed than is the passage of the tidal-wave itself. "Among the hundreds of ships which were sailing upon the Pacific when its length and breadth

were traversed by the great sea-wave, there was not one in which any unusual motion was perceived." The inference is clear, that the slope of the advancing and following faces of the great wave was very much less than in the case above imagined; in other words, that the breadth of the wave greatly exceeded one mile—amounting, in fact, to many miles.

Where the interval between the passage of successive wave-crests was noted, we can tell the actual breadth of the wave. Thus, at the Samoan Isles, in 1868, the crests succeeded each other at intervals of sixteen minutes, corresponding to eight minutes between crest and trough. But we have seen, that if the waves were one mile in breadth, the corresponding interval would be only four seconds, or only one  $\frac{1}{120}$ th part of eight minutes: it follows, then, that the breadth of the great wave, where it reached the Samoan Isles in 1868, was about 120 miles.

Now a wave extending right athwart the Pacific Ocean, and having a cross breadth of more than 120 miles, would be discernible as a marked feature of the disc of our earth, seen under the conditions described above, either from Mercury or Venus. It is true that the slope of the wave's advancing and following surfaces would be but slight, yet the difference of the illumination under the sun's rays would be recognizable. Then, also, it is to be remembered that there was not merely a single wave, but a succession of many waves. These travelled also with enormous velocity; and though at the distance of even the nearest planet, the apparent motion of the great wave, swift though it was in reality, would be so far reduced that it would have to be estimated rather than actually seen, yet there would be no difficulty in thus perceiving it with the mind's eye. The rate of motion indeed would almost be exactly the same as that of the equatorial part of the surface of Mars, in consequence of the planet's rotation; and this (as is well known to telescopists), though not discernible directly, produces, even in a few minutes, changes which a good eye can clearly recognize.

We can scarcely doubt then that if our earth were so situated at any time when one of the great waves generated by Peruvian earthquakes in traversing the Pacific, that the hemisphere containing this ocean were turned fully illuminated towards Venus (favourably placed for observing her), the disturbance of the Pacific could be observed and measured by telescopists on that planet.

Unfortunately there is little chance that terrestrial observers will ever be able to watch the progress of great waves athwart the oceans of Mars, and still less that any disturbance of the frame of Venus should become discernible to us by its effects. We can scarce even be assured that there are lands and seas on Venus, so far as direct observation is concerned, so unfavourably is she always placed for observation ; and though we see Mars under much more favourable conditions, his seas are too small and would seem to be too shallow (compared with our own) for great waves to traverse them such as could be discerned from the earth.

Yet it is well to remember the possibility that changes may at times take place in the nearer planets—the terrestrial planets, as they are commonly called, Mars, Venus, and Mercury—such as telescopic observation under favourable conditions might detect. Telescopists have, indeed, described apparent changes, lasting only for a short time, in the appearance of one of these planets, Mars, which may fairly be attributed to disturbances affecting its surface in no greater degree than the great Peruvian earthquakes have affected for a time the surface of our earth. For instance, the American astronomer Mitchel says that on the night of July 12, 1845, the bright polar snows of Mars exhibited an appearance never noticed at any preceding or succeeding observation. In the very centre of the white surface appeared a dark spot, which retained its position during several hours : on the following evening not a trace of the spot could be seen. Again the same observer says that on the evening of August 30, 1845, he observed for the first time a small bright spot, nearly or quite round, projecting out of the

lower side of the polar spot. "In the early part of the evening," he says, "the small bright spot seemed to be partly buried in the large one. After the lapse of an hour or more my attention was again directed to the planet, when I was astonished to find a manifest change in the position of the small bright spot. It had apparently separated from the large spot, and the edges alone of the two were now in contact, whereas when first seen they overlapped by an amount quite equal to one-third of the diameter of the small one. This, however, was merely an optical phenomenon, for on the next evening the spots went through the same apparent changes as the planet went through the corresponding part of its rotation. But it showed the spots to be real ice masses. The strange part of the story is that in the course of a few days the smaller spot, which must have been a mass of snow and ice as large as Novaia Zemlia, gradually disappeared. Probably some great shock had separated an enormous field of ice from the polar snows, and it had eventually been broken up and its fragments carried away from the Arctic regions by currents in the Martian oceans. It appears to me that the study of our own earth, and of the changes and occasional convulsions which affect its surface, gives to the observation of such phenomena as I have just described a new interest. Or rather, perhaps, it is not too much to say that the telescopic observations of the planets derive their only real interest from such considerations.

I may note in conclusion, that while on the one hand we cannot doubt that the earth is slowly parting with its internal heat, and thus losing century by century a portion of its Vulcanian energy, such phenomena as the Peruvian earthquakes show that the loss of energy is taking place so slowly that the diminution during many ages is almost imperceptible. As I have elsewhere remarked, "When we see that while mountain ranges were being upheaved or valleys depressed to their present position, race after race and type after type lived out on the earth the long lives which belong to races and to types, we recognize the great work which the

earth's subterranean forces are still engaged upon. Even now continents are being slowly depressed or upheaved, even now mountain ranges are being raised to a different level, table-lands are being formed, great valleys are being gradually scooped out; old shore-lines shift their place, old soundings vary; the sea advances in one place and retires in another; on every side nature's plastic hand is still at work, modelling and remodelling the earth, and making it constantly a fit abode for those who dwell upon it."

## *STRANGE SEA CREATURES.*

"We ought to make up our minds to dismiss as idle prejudices, or, at least, suspend as premature, any preconceived notion of what *might*, or what *ought to*, be the order of nature, and content ourselves with observing, as a plain matter of fact, what *is*."—Sir J. HERSCHEL, "Prelim. Disc." page 79.

THE fancies of men have peopled three of the four so-called elements, earth, air, water, and fire, with strange forms of life, and have even found in the salamander an inhabitant for the fourth. On land the centaur and the unicorn, in the air the dragon and the roc, in the water tritons and mermaids, may be named as instances among many of the fabulous creatures which have been not only imagined but believed in by men of old times. Although it may be doubted whether men have ever invented any absolutely imaginary forms of life, yet the possibility of combining known forms into imaginary, and even impossible, forms, must be admitted as an important element in any inquiry into the origin of ideas respecting such creatures as I have named. One need only look through an illuminated manuscript of the Middle Ages to recognize the readiness with which imaginary creatures can be formed by combining, or by exaggerating, the characteristics of known animals. Probably the combined knowledge and genius of all the greatest zoologists of our time would not suffice for the invention of an entirely new form of animal which yet should be zoologically possible; but to combine the qualities of several existent animals in a single one, or to conceive an

animal with some peculiarity abnormally developed, is within the capacity of persons very little acquainted with zoology, nay, is perhaps far easier to such persons than it would be to an Owen, a Huxley, or a Darwin. In nearly every case, however, the purely imaginary being is to be recognized by the utter impossibility of its actual existence. If it be a winged man, arms and wings are both provided, but the pectoral muscles are left unchanged. A winged horse, in like manner, is provided with wings, without any means of working them. A centaur, as in the noble sculptures of Phidias, has the upper part of the trunk of a man superadded, not to the hind quarters of a horse or other quadruped, but to the entire trunk of such an animal, so that the abdomen of the human figure lies *between* the upper half of the human trunk and the corresponding part of the horse's trunk, an arrangement anatomically preposterous. Without saying that every fabulous animal which was anatomically and zoologically possible, had a real antitype, exaggerated though the fabulous form may have been, we must yet admit that errors so gross marked the conception of all the really imaginary animals of antiquity, that any fabulous animal found to accord fairly well with zoological possibilities may be regarded, with extreme probability, as simply the exaggerated presentation of some really existent animal. The inventors of centaurs, winged and man-faced bulls, many-headed dogs, harpies, and so forth, were utterly unable to invent a possible new animal, save by the merest chance, the probability of which was so small that it may fairly be disregarded.

This view of the so-called fabulous animals of antiquity has been confirmed by the results of modern zoological research. The merman, zoologically possible (not in all details, of course, but generally), has found its antitype in the dugong and the manatee; the roc in the condor, or perhaps in those extinct species whose bones attest their monstrous proportions; the unicorn in the rhinoceros; even the dragon in the pterodactyl of the green-sand; while the

centaur, the minotaur, the winged horse, and so forth, have become recognized as purely imaginary creatures, which had their origin simply in the fanciful combination of known forms, no existent creatures having even suggested these monstrosities.

It is not to be wondered at that the sea should have been more prolific in monstrosities and in forms whose real nature has been misunderstood. Land animals cannot long escape close observation. Even the most powerful and ferocious beasts must succumb in the long run to man, and in former ages, when the struggle was still undecided between some race of animals and savage man, individual specimens of the race must often have been killed, and the true appearance of the animal determined. Powerful winged animals might for a longer time remain comparatively mysterious creatures even to those whom they attacked, or whose flocks they ravaged. A mighty bird, or a pterodactylian creature (a late survivor of a race then fast dying out), might swoop down on his prey and disappear with it too swiftly to be made the subject of close scrutiny, still less of exact scientific observation. Yet the general characteristics even of such creatures would before long be known. From time to time the strange winged monster would be seen hovering over the places where his prey was to be found. Occasionally it would be possible to pierce one of the race with an arrow or a javelin; and thus, even in those remote periods when the savage progenitors of the present races of man had to carry on a difficult contest with animals now extinct or greatly reduced in power, it would become possible to determine accurately the nature of the winged enemy. But with sea creatures, monstrous, or otherwise, the case would be very different. To this day we remain ignorant of much that is hidden beneath the waves of the "hollow-sounding and mysterious main." Of far the greater number of sea creatures, it may truly be said that we never see any specimens except by accident, and never obtain the body of any except by very rare accident. Those creatures of the

deep sea which we are best acquainted with, are either those which are at once very numerous and very useful as food or in some other way, or else those which are very rapacious and thus expose themselves, by their attacks on men, to counter-attack and capture or destruction. In remote times, when men were less able to traverse the wide seas, when, on the one hand, attacks from great sea creatures were more apt to be successful, while, on the other, counter-attack was much more dangerous, still less would be known about the monsters of the deep. Seen only for a few moments as he seized his prey, and then sinking back into the depths, a sea monster would probably remain a mystery even to those who had witnessed his attack, while their imperfect account of what they had seen would be modified at each repetition of the story, until there would remain little by which the creature could be identified, even if at some subsequent period its true nature were recognized. We can readily understand, then, that among the fabulous creatures of antiquity, even of those which represented actually existent races incorrectly described, the most remarkable, and those zoologically the least intelligible, would be the monsters of the deep sea. We can also understand that even the accounts which originally corresponded best with the truth would have undergone modifications much more noteworthy than those affecting descriptions of land animals or winged creatures—simply because there would be small chance of any errors thus introduced being corrected by the study of freshly discovered specimens.

We may, perhaps, explain in this way the strange account given by Berossus of the creature which came up from the Red Sea, having the body of a fish but the front and head of a man. We may well believe that this animal was no other than a dugong, or halicore (a word signifying sea-maiden), a creature inhabiting the Indian Ocean to this day, and which might readily find its way into the Red Sea. But the account of the creature has been strangely altered from the original narrative, if at least the original narrative was correct. For,

according to Berosus, the animal had two human feet which projected from each side of the tail ; and, still stranger, it had a human voice and human language. " This strange monster sojourned among the rude people during the day, taking no food, but retiring to the sea again at night, and continued for some time teaching them the arts of civilized life." A picture of this stranger is said to have been preserved at Babylon for many centuries. With a probable substratum of truth, the story in its latest form is as fabulous as Autolycus's " ballad of a fish that appeared upon the coast, on Wednesday the fourscore of April, forty thousand fathoms above water, and sang a ballad against the hard hearts of maids."

It is singular, by the way, how commonly the power of speech, or at least of producing sounds resembling speech or musical notes, was attributed to the creature which imagination converted into a man-fish or woman-fish. Dugongs and manatees make a kind of lowing noise, which could scarcely be mistaken under ordinary conditions for the sound of the human voice. Yet, not only is this peculiarity ascribed to the mermaid and siren (the merman and triton having even the supposed power of blowing on conch-shells), but in more recent accounts of encounters with creatures presumably of the seal tribe and allied races, the same feature is to be noticed. The following account, quoted by Mr. Gosse from a narrative by Captain Weddell, the well-known geographer, is interesting for this reason amongst others. It also illustrates well the mixture of erroneous details (the offspring, doubtless, of an excited imagination) with the correct description of a sea creature actually seen :—" A boat's crew were employed on Hall's Island, when one of the crew, left to take care of some produce, saw an animal whose voice was musical. The sailor had lain down, and at ten o'clock he heard a noise resembling human cries, and as daylight in these latitudes never disappears at this season" (the Antarctic summer), " he rose and looked around, but, on seeing no person, returned to bed. Presently he heard

the noise again ; he rose a second time, but still saw nothing. Conceiving, however, the possibility of a boat being upset, and that some of the crew might be clinging to detached rocks, he walked along the beach a few steps and heard the noise more distinctly but in a musical strain. Upon searching around, he saw an object lying on a rock a dozen yards from the shore, at which he was somewhat frightened. The face and shoulders appeared of human form and of a reddish colour ; over the shoulders hung long green hair ; the tail resembled that of the seal, but the extremities of the arms he could not see distinctly. The creature continued to make a musical noise while he gazed about two minutes, and on perceiving him it disappeared in an instant. Immediately, when the man saw his officer, he told this wild tale, and to add weight to his testimony (being a Romanist) he made a cross on the sand, which he kissed, as making oath to the truth of his statement. When I saw him he told the story in so clear and positive a manner, making oath to its truth, that I concluded he must really have seen the animal he described, or that it must have been the effects of a disturbed imagination."

In this story all is consistent with the belief that the sailor saw an animal belonging to the seal family (of a species unknown to him), except the green hair. But the hour was not very favourable to the discerning of colour, though daylight had not quite passed away, and as Gosse points out, since golden-yellow fur and black fur are found among Antarctic seals, the colours may be intermingled in some individuals, producing an olive-green tint, which, by contrast with the reddish skin, might be mistaken for a full green. Considering that the man had been roused from sleep and was somewhat frightened, he would not be likely to make very exact observations. It will be noticed that it was only at first that he mistook the sounds made by the creature for human cries ; afterwards he heard only the same *noise*, but in a musical strain. Now with regard to the musical sounds said to have been uttered by this creature,

and commonly attributed to creatures belonging to families closely allied to the seals, I do not know that any attempt has yet been made to show that these families possess the power of emitting sounds which can properly be described as musical. It is quite possible that the Romanist sailor's ears were not very nice, and that any sound softer than a bellow seemed musical to him. Still, the idea suggests itself that possibly seals, like some other animals, possess a note not commonly used, but only as a signal to their mates, and never uttered when men or other animals are known to be near. It appears to me that this is rendered probable by the circumstance that seals are fond of music. Darwin refers to this in his treatise on Sexual Selection (published with his "Descent of Man"), and quotes a statement to the effect that the fondness of seals for music "was well known to the ancients, and is often taken advantage of by hunters to the present day." The significance of this will be understood from Darwin's remark immediately following, that "with all these animals, the males of which during the season of courtship incessantly produce musical notes or mere rhythmical sounds, we must believe that the females are able to appreciate them."

The remark about the creature's arms seems strongly to favour the belief that the sailor intended his narrative to be strictly truthful. Had he wished to excite the interest of his comrades by a marvellous story, he certainly would have described the creature as having well-developed human hands.

Less trustworthy by far seem some of the stories which have been told of animals resembling the mermaid of antiquity. It must always be remembered, however, that in all probability we know very few among the species of seals and allied races, and that some of these species may present, in certain respects and perhaps at a certain age, much closer resemblance to the human form than the sea-lion, seal, manatee, or dugong.

We cannot, for instance, attach much weight to the fol-

lowing story related by Hudson, the famous navigator :— “One of our company, looking overboard, saw a mermaid and calling up some of the company to see her, one more came up, and by that time she was come close to the ship’s side, looking earnestly on the men. A little after a sea came and overturned her. From the navel upward her back and breasts were like a woman’s, as they say that saw her ; her body as big as one of us ; her skin very white ; and long hair hanging down behind, of colour black. In her going down they saw her tail, which was like the tail of a porpoise and speckled like a mackerel.” If Hudson himself had seen and thus described the creature it would have been possible to regard the story with some degree of credence ; but his account of what Thomas Hilles and Robert Rayner, men about whose character for veracity we know nothing, *said* they saw, is of little weight. The skin very white, and long hair hanging down behind, are especially suspicious features of the narrative ; and were probably introduced to dispose of the idea, which others of the crew may have advanced, that the creature was only some kind of seal after all. The female seal (*Phoca Greenlandica* is the pretty name of the animal) is not, however, like the male, tawny grey, but dusky white, or yellowish straw-colour, with a tawny tint on the back. The young alone could be called “very white.” They are so white in fact as scarcely to be distinguishable when lying on ice and snow, a circumstance which, as Darwin considers, serves as a protection for these little fellows.

The following story, quoted by Gosse from Dr. Robert Hamilton’s able “History of the Whales and Seals,” compares favourably in some respects with the last narrative :—“It was reported that a fishing-boat off the island of Yell, one of the Shetland group, had captured a mermaid by its getting entangled in the lines ! The statement is, that the animal is about three feet long, the upper part of the body resembling the human, with protuberant mammae like a woman ; the face, the forehead, and neck, were short, and resembling

those of a monkey ; the arms, which were small, were kept folded across the breast ; the fingers were distinct, not webbed ; a few stiff long bristles were on the top of the head, extending down to the shoulders, and these it could erect and depress at pleasure, something like a crest. The inferior part of the body was like a fish. The skin was smooth and of a grey colour. It offered no resistance, nor attempted to bite, but uttered a low, plaintive sound. The crew, six in number, took it within their boat ; but superstition getting the better of curiosity, they carefully disentangled it from the lines and a hook which had accidentally fastened in the body, and returned it to its native element. It instantly dived, descending in a perpendicular direction." "They had the animal for three hours within the boat ; the body was without scales or hair ; of a silvery grey colour above, and white below, like the human skin ; no gills were observed, nor fins on the back or belly. The tail was like that of the dog-fish ; the mammae were about as large as those of a woman ; the mouth and lips were very distinct, and resembled the human."

This account, if accepted in all its details, would certainly indicate that an animal of some species before unknown had been captured. But it is doubtful how much reliance can be placed on the description of the animal. Mr. Gosse, commenting upon the case, says that the fishermen cannot have been affected by fear in such sort that their imagination exaggerated the resemblance of the creature to the human form. "For the mermaid," he says, "is not an object of terror to the fishermen ; it is rather a welcome guest, and danger is to be apprehended only from its experiencing bad treatment." But then this creature had not been treated as a specially welcome guest. The crew had captured it ; and probably not without some degree of violence ; for though it offered no resistance it uttered a plaintive cry. And that hook which "had accidentally fastened in the body" has a very suspicious look. If the animal could have given its own account of the capture, probably the hook

would not have been found to have fastened in the body altogether by accident. Be this as it may, the fishermen were so far frightened that superstition got the better of curiosity ; so that, as they were evidently very foolish fellows, their evidence is scarcely worth much. There are, however, only two points in their narrative which do not seem easily reconciled with the belief that they had captured a rather young female of a species closely allied to the common seal —the distinct unwebbed fingers and the small arms folded across the breast. Other points in their description suggest marked differences in degree from the usual characteristics of the female seal ; but these two alone seem to differ absolutely in kind. Considering all the circumstances of the narrative, we may perhaps agree with Mr. Gosse to this extent, that, combined with other statements, the story induces a strong suspicion that the northern seas may hold forms of life as yet uncatalogued by science.

The stories which have been related about monstrous cuttle-fish have only been fabulous in regard to the dimensions which they have attributed to these creatures. Even in this respect it has been shown, quite recently, that some of the accounts formerly regarded as fabulous fell even short of the truth. Pliny relates, for instance, that the body of a monstrous cuttle-fish, of a kind known on the Spanish coast, weighed, when captured, 700 lbs., the head the same, the arms being 30 feet in length. The entire weight would probably have amounted to about 2000 lbs. But we shall presently see that this weight has been largely exceeded by modern specimens. It was, however, in the Middle Ages that the really fabulous cuttle-fish flourished—the gigantic kraken, “liker an island than an animal,” according to credulous Bishop Pontoppidan, and able to destroy in its mighty arms the largest galleons and war-ships of the fourteenth and fifteenth centuries.

It is natural that animals really monstrous should be magnified by the fears of those who have seen or encountered them, and still further magnified afterwards by tradi-

tion. Some specimens of cuttle-fish which have been captured wholly, or in part, indicate that this creature sometimes attains such dimensions that but little magnifying would be needed to suggest even the tremendous proportions of the fabulous kraken. In 1861, the French war-steamer *Alecton* encountered a monstrous cuttle, on the surface of the sea, about 120 miles north-east of Teneriffe. The crew succeeded in slipping a noose round the body, but unfortunately the rope slipped, and, being arrested by the tail fin, pulled off the tail. This was hauled on board, and found to weigh over 40 lbs. From a drawing of the animal, the total length without the arms was estimated at 50 feet, and the weight at 4000 lbs., nearly twice the weight of Pliny's monstrous cuttle-fish, long regarded as fabulous. In one respect this creature seems to have been imperfect, the two long arms usually possessed by cuttle-fish of the kind being wanting. Probably it had lost these long tentacles in a recent encounter with some sea enemy, perhaps one of its own species. Quite possibly it may have been such recent mutilation which exposed this cuttle-fish to successful attack by the crew of the *Alecton*.

A cuttle-fish of about the same dimensions was encountered by two fishermen in 1873, in Conception Bay, Newfoundland. When they attacked it, the creature threw its long arms across the boat, but the fishermen with an axe cut off these tentacles, on which the cephalopod withdrew in some haste. One of the arms was preserved, after it had lost about 6 feet of its length. Even thus reduced it measured 19 feet; and as the fishermen estimate that the arm was struck off about 10 feet from the body, it follows that the entire length of the limb must have been about 35 feet. They estimated the body at 60 feet in length and 5 feet in diameter—a monstrous creature! It was fortunate for these fishermen that they had an axe handy for its obtrusive tentacles, as with so great a mass and the great propulsive power possessed by all cephalopods, it might readily have upset their small boat. Once in the water, they would have

been at the creature's mercy—a quality which, by all accounts, the cuttle-fish does not possess to any remarkable extent.

Turn we, however, from the half fabulous woman-fish, and the exaggeratedly monstrous cuttle-fish, to the famous sea-serpent, held by many to be the most utterly fabulous of all fabled creatures, while a few, including some naturalists of distinction, stoutly maintain that the creature has a real existence, though whether it be rightly called a sea-serpent or not is a point about which even believers are extremely doubtful.

It may be well, in the first place, to remark that in weighing the evidence for and against the existence of this creature, and bearing on the question of its nature (if its existence be admitted), we ought not to be influenced by the manifest falsity of a number of stories relating to supposed encounters with this animal. It is probable that, but for these absurd stories, the well-authenticated narratives relating to the creature, whatever it may be, which has been called the sea-serpent, would have received much more attention than has heretofore been given to them. It is also possible that some narratives would have been published which have been kept back from the fear lest a truthful (though possibly mistaken) account should be classed with the undoubted untruths which have been told respecting the great sea-serpent. It cannot be denied that in the main the inventions and hoaxes about the sea-serpent have come chiefly from American sources. It is unfortunately supposed by too many of the less cultured sons of America that (to use Mr. Gosse's expression) "there is somewhat of wit in gross exaggerations or hoaxing inventions." Of course an American gentleman—using the word "in that sense in which every man may be a gentleman," as Twemlow hath it—would as soon think of uttering a base coin as a deliberate untruth or foolish hoax. But it is thought clever, by not a few in America who know no better, to take any one in by an invention. Some, perhaps but a small number, of the news-

papers set a specially bad example in this respect, giving room in their columns for pretended discoveries in various departments of science, elaborate accounts of newly discovered animals, living or extinct, and other untruths which would be regarded as very disgraceful indeed by English editors. Such was the famous "lunar hoax," published in the New York *Sun* some forty years ago ; such the narrative, in 1873, of a monstrous fissure which had been discerned in the body of the moon, and threatened to increase until the moon should be cloven into two unequal parts ; such the fables which have from time to time appeared respecting the sea-serpent. But it would be as unreasonable to reject, because of these last-named fables, the narratives which have been related by quiet, truth-loving folk, and have borne close and careful scrutiny, as it would be to reject the evidence given by the spectroscope respecting the existence of iron and other metals in the sun because an absurd story had told how creatures in the moon had been observed to make use of metal utensils or to adorn the roofs of their temples with metallic imitations of wreathed flames.

The oldest accounts on record of the appearance of a great sea creature resembling a serpent are those quoted by Bishop Pontoppidan, in his description of the natural history of his native country, Norway. Amongst these was one confirmed by oath taken before a magistrate by two of the crew of a ship commanded by Captain de Ferry, of the Norwegian navy. The captain and eight men saw the animal, near Molde, in August, 1747. They described it as of the general form of a serpent, stretched on the surface in receding coils (meaning, probably, the shape assumed by the neck of a swan when the head is drawn back). The head, which resembled that of a horse, was raised two feet above the water.

In August, 1817, a large marine animal, supposed to be a serpent, was seen near Cape Ann, Massachusetts. Eleven witnesses of good reputation gave evidence on oath before magistrates. One of these magistrates had himself seen the

creature, and corroborated the most important points of the evidence given by the eleven witnesses. The creature had the appearance of a serpent, dark brown in colour (some said mottled), with white under the head and neck. Its length was estimated at from 50 to 100 feet. The head was in shape like a serpent's, but as large as a horse's. No mane was noticed. Five of the witnesses deposed to protuberances on the back; four said the back was straight; the other two gave no opinion on this point. The magistrate who had seen the animal considered the appearance of protuberances was due to the bendings of the body while in rapid motion.

In 1848, when the captain of the British frigate *Dædalus* had published an account of a similar animal seen by him and several of his officers and crew, the Hon. Colonel T. H. Perkins, of Boston, who had seen the animal on the occasion just mentioned in 1817, gave an account (copied from a letter written in 1820) of what he had witnessed. It is needless to quote those points which correspond with what has been already stated. Colonel Perkins noticed "an appearance in the front of the head like a single horn, about nine inches to a foot in length, shaped like a marlinspike, which will presently be explained. I left the place," he proceeds, "fully satisfied that the reports in circulation, though differing in details, were essentially correct." He relates how a person named Mansfield, "one of the most respectable inhabitants of the town, who had been such an unbeliever in the existence of this monster that he had not given himself the trouble to go from his house to the harbour when the report was first made," saw the animal from a bank overlooking the harbour. Mr. Mansfield and his wife agreed in estimating the creature's length at 100 feet. Several crews of coasting vessels saw the animal, *in some instances within a few yards*. "Captain Tappan," proceeds Colonel Perkins, "a person well known to me, saw him with his head above water two or three feet, at times moving with great rapidity, at others slowly. He also saw what explained the

appearance which I have described of a horn on the front of the head. This was doubtless what was observed by Captain Tappan to be the tongue, thrown in an upright position from the mouth, and having the appearance which I have given to it. One of the revenue cutters, whilst in the neighbourhood of Cape Ann, had an excellent view of him at a few yards' distance ; he moved slowly, and upon the appearance of the vessel sank and was seen no more."

Fifteen years later, in May 1833, five British officers—Captain Sullivan, Lieutenants MacLachlan and Malcolm of the Rifle Brigade, Lieutenant Lyster of the Artillery, and Mr. Snee of the Ordnance—when cruising in a small yacht off Margaret's Bay, not far from Halifax, "saw the head and neck of some denizen of the deep, precisely like those of a common snake, in the act of swimming, the head so elevated and thrown forward by the curve of the neck as to enable us to see the water under and beyond it." They judged its length to exceed 80 feet. "There could be no mistake nor delusion, and we were all perfectly satisfied that we had been favoured with a view of the 'true and veritable sea-serpent,' which had been generally considered to have existed only in the brain of some Yankee skipper, and treated as a tale not entitled to belief." Dowling, a man-of-war's man they had along with them, made the following unscientific but noteworthy comment : "Well, I've sailed in all parts of the world, and have seen rum sights too in my time, but this is the queerest thing I ever see." "And surely," adds Captain Sullivan, "Jack Dowling was right." The description of the animal agrees in all essential respects with previous accounts, but the head was estimated at about six feet in length—considerably larger, therefore, than a horse's head.

But unquestionably the account of the sea-serpent which has commanded most attention was that given by the captain, officers, and crew of the British frigate *Dædalus*, Captain M'Quhae, in 1848. The *Times* published on October 9, 1848, a paragraph stating that the sea-serpent had been seen

by the captain and most of the officers and crew of this ship, on her passage home from the East Indies. The Admiralty inquired at once into the truth of the statement, and the following is abridged from Captain M'Quhae's official reply, addressed to Admiral Sir W. H. Gage.

"Sir,—In reply to your letter, requiring information as to the truth of a statement published in the *Times* newspaper, of a sea-serpent of extraordinary dimensions having been seen from the *Dædalus*, I have the honour to inform you that at 5 p.m., August 6 last, in latitude  $24^{\circ} 44'$  S., longitude  $9^{\circ} 22'$  E., the weather dark and cloudy, wind fresh from N.W., with long ocean swell from S.W., the ship on the port tack, heading N.E. by N., Mr. Sartoris, midshipman, reported to Lieutenant E. Drummond (with whom, and Mr. W. Barrett, the master, I was walking the quarter-deck) something very unusual rapidly approaching the ship from before the beam. The object was seen to be an enormous serpent, with head and shoulders kept about four feet constantly above the surface of the sea, as nearly as we could judge; at least 60 feet of the animal was on the surface, no part of which length was used, so far as we could see, in propelling the animal either by vertical or horizontal undulation. It passed quietly, *but so closely under our lee quarter that, had it been a man of my acquaintance, I should easily have recognized his features with the naked eye.* It did not, while visible, deviate from its course to the S.W., which it held on at the pace of from 12 to 15 miles per hour, as if on some determined purpose. The diameter of the serpent was from 15 to 16 inches behind the head, which was, without any doubt, that of a snake. Its colour was a dark brown, with yellowish white about the throat. It did not once, while within the range of view from our glasses, sink below the surface. It had no fins, but something like the mane of a horse, or rather a bunch of sea-weed, washed about its back. It was seen by the quarter-master, the boatswain's mate, and the man at the wheel, in addition to myself and the officers above-

mentioned. I am having a drawing of the serpent made from a sketch taken immediately after it was seen, which I hope to have ready for my Lords Commissioners of the Admiralty by to-morrow's post.—Peter M'Quhæ, Captain."

The drawing here mentioned was published in the *Illustrated London News* for October 28, 1848, being there described as made "under the supervision of Captain M'Quhæ, and his approval of the authenticity of the details as to position and form."

The correspondence and controversy elicited by the statement of Captain M'Quhæ were exceedingly interesting. It is noteworthy, at the outset, that few, perhaps none, who had read the original statement, suggested the idea of illusion, while it need hardly perhaps be said that no one expressed the slightest doubt as to the *bona fides* of Captain M'Quhæ and his fellow-witnesses. These points deserve attention, because, in recent times, the subject of the sea-serpent has been frequently mentioned in public journals and elsewhere as though no accounts of the creature had ever been given which had been entitled to credence. I proceed to summarise the correspondence which followed M'Quhæ's announcement. The full particulars will be found in Mr. Gosse's interesting work, the "Romance of Natural History," where, however, as it seems to me, the full force of the evidence is a little weakened, for all save naturalists, by the introduction of particulars not bearing directly on the questions at issue.

Among the earliest communications was one from Mr. J. D. M. Stirling, a gentleman who, during a long residence in Norway, had heard repeated accounts of the sea-serpent in Norwegian seas, and had himself seen a fish or reptile at a distance of a quarter of a mile, which, examined through a telescope, corresponded in appearance with the sea-serpent as usually described. This communication was chiefly interesting, however, as advancing the theory that the supposed sea-serpent is not a serpent at all, but a long-necked plesiosaurian. This idea had been advanced earlier, but with-

out his knowledge, by Mr. E. Newman, the editor of the *Zoologist*. Let us briefly inquire into the circumstances which suggest the belief.

If we consider the usual account of the sea-serpent, we find one constant feature, which seems entirely inconsistent with the belief that the creature can be a serpent. The animal has always shown a large portion of its length, from 20 to 60 feet, above the surface of the water, and without any evident signs of undulation, either vertically or horizontally. Now, apart from all zoological evidence, our knowledge of physical laws will not permit us to believe that the portion thus visible above the surface was propelled by the undulations of a portion concealed below the surface, unless this latter portion largely exceeded the former in bulk. A true fish does not swim for any length of time with any but a very small portion of its body above water; probably large eels never show even a head or fin above water for more than a few seconds when not at rest. Cetaceans, owing to the layers of blubber which float them up, remain often for a long time with a portion of their bulk out of the water, and the larger sort often swim long distances with the head and fore-part out of water. But, even then, the greater part of the creature's bulk is under water, and the driving apparatus, the anterior fins and the mighty tail, are constantly under water (when the animal is urging its way horizontally, be it understood). A sea creature, in fact, whatever its nature, which keeps any considerable volume of its body out of water constantly, while travelling a long distance, must of necessity have a much greater volume all the time under water, and must have its propelling apparatus under water. Moreover, if the propulsion is not effected by fins, paddles, a great flat tail, or these combined, but by the undulations of the animal's own body, then the part out of water must of necessity be affected by these undulations, unless it is very small in volume and length compared with the part under water. I assert both these points as matters depending on physical laws, and without fear that the best-informed

zoologist can adduce any instances to the contrary. It is in fact physically impossible that such instances should exist.

It would not be saying too much to assert that if the so-called sea-serpent were really a serpent, its entire length must be nearer 1000 than 100 feet. This, of course, is utterly incredible. We are, therefore, forced to the belief that the creature is not a serpent. If it were a long-necked reptile, with a concealed body much bulkier than the neck, the requirements of floatation would be satisfied ; if to that body there were attached powerful paddles, the requirements of propulsion would be satisfied. The theory, then, suggested, first by Mr. Newman, later but independently by Mr. Stirling, and advocated since by several naturalists of repute, is simply that the so-called sea-serpent is a modern representative of the long-necked plesiosaurian reptile to which has been given the name of the *enaliosaurus*. Creatures of this kind prevailed in that era when what is called the lias was formed, a fossiliferous stratum belonging to the secondary or mesozoic rocks. They are not found in the later or tertiary rocks, and thereon an argument might be deduced against their possible existence in the present, or post-tertiary, period ; but, as will presently be shown, this argument is far from being conclusive. The enaliosaurian reptiles were "extraordinary," says Lyell, "for their number, size, and structure." Like the ichthyosauri, or fish-lizards, the enaliosauri (or serpent-turtles, as they might almost be called) were carnivorous, their skeletons often enclosing the fossilized remains of half-digested fishes. They had extremely long necks, with heads very small compared with the body. They are supposed to have lived chiefly in narrow seas and estuaries, and to have breathed air like the modern whales and other aquatic mammals. Some of them were of formidable dimensions, though none of the skeletons yet discovered indicate a length of more than 35 feet. It is not, however, at all likely that the few skeletons known indicate the full size attained by these creatures. Probably, indeed, we have the remains of only a few out of many species, and some

species existing in the mesozoic period may have as largely exceeded those whose skeletons have been found, as the boa-constrictor exceeds the common ringed snake. It is also altogether probable that in the struggle for existence during which the enaliosaurian reptiles have become *almost* extinct (according to the hypothesis we are considering), none but the largest and strongest had any chance, in which case the present representatives of the family would largely exceed in bulk their progenitors of the mesozoic period.

A writer in the *Times* of November 2, 1848, under the signature F. G. S., pointed out how many of the external characters of the creature seen from the *Dædalus* corresponded with the belief that it was a long-necked plesiosaurus. "Geologists," he said, "are agreed in the inference that the plesiosauri carried their necks, which must have resembled the bodies of serpents, above the water, while their propulsion was effected by large paddles working beneath, the short but stout tail acting the part of a rudder. . . . In the letter and drawing of Captain M'Quhæ . . . . we have . . . . the short head, the serpent-like neck, carried several feet above the water. Even the bristly mane in certain parts of the back, so unlike anything found in serpents, has its analogue in the iguana, to which animal the plesiosaurus has been compared by some geologists. But I would most of all insist upon the peculiarity of the animal's progression, which could only have been effected with the evenness and at the rate described by an apparatus of fins or paddles, not possessed by serpents, but existing in the highest perfection in the plesiosaurus."

At this stage a very eminent naturalist entered the field—Professor Owen. He dwelt first on a certain characteristic of Captain M'Quhæ's letter which no student of science could fail to notice—the definite statement that the creature *was* so and so, where a scientific observer would simply have said that the creature presented such and such characteristics. "No sooner was the captain's attention called to the object," says Professor Owen, "than 'it was discovered.'

to be an enormous serpent,'" though in reality the true nature of the creature could not be determined even from the observations made during the whole time that it remained visible. Taking, however, "the more certain characters," the "head with a convex, moderately capacious cranium, short, obtuse muzzle, gape not extending further than to beneath the eye, which (the eye) is rather small, round, filling closely the palpebral aperture" (that is, the eyelids fit closely \*); "colour and surface as stated; nostrils indicated in the drawing by a crescentic mark at the end of the nose or muzzle. All these," proceeds Owen, "are the characters of the head of a warm-blooded mammal, none of them those of a cold-blooded reptile or fish. Body long, dark brown, not undulating, without dorsal or other apparent fins, 'but something like the mane of a horse, or rather a bunch of sea-weed, washed about its back.'" He infers that the creature had hair, showing only where longest on the back, and therefore that the animal was not a mammal of the whale species but rather a great seal. He then shows that the sea-elephant, or *Phoca proboscidea*, which attains the length of from 20 to 30 feet, was the most probable member of the seal family to be found about 300 miles from the western shore of the southern end of Africa, in latitude  $24^{\circ} 44'$ . Such a creature, accidentally carried from its natural domain by a floating iceberg, would have (after its iceberg had melted) to urge its way steadily southwards, as the supposed sea-serpent was doing; and probably the creature approached the *Dædalus* to scan her "capabilities as a resting-place, as it paddled its long, stiff body past the ship." "In so doing it would raise a head of the form and colour described and delineated by Captain M'Quhæ"—its head only, be it remarked, corresponding with the captain's description. The neck also would be of the right diameter.

\* It is a pity that men of science so often forget, when addressing those who are not men of science, or who study other departments than theirs, that technical terms are out of place. Most people, I take it, are more familiar, on the whole, with eyelids than with *palpebrae*.

The thick neck, passing into an inflexible trunk, the longer and coarser hair on the upper part of which would give rise to the idea "explained by the similes above cited" (of a mane or bunch of sea-weed), the paddles would be out of sight ; and the long eddy and wake created by the propelling action of the tail would account for the idea of a long serpentine body, at least for this idea occurring to one "looking at the strange phenomenon with a sea-serpent in his mind's eye." "It is very probable that not one on board the *Dædalus* ever before beheld a gigantic seal freely swimming in the open ocean." The excitement produced by the strange spectacle, and the recollection of "old Pontoppidan's sea-serpent with the mane," would suffice, Professor Owen considered, to account for the metamorphosis of a sea-elephant into a maned sea-serpent.

This was not the whole of Professor Owen's argument ; but it may be well to pause here, to consider the corrections immediately made by Captain M'Quhae ; it may be noticed, first, that Professor Owen's argument seems sufficiently to dispose of the belief that the creature really was a sea-serpent, or any cold-blooded reptile. And this view of the matter has been confirmed by later observations. But few, I imagine, can readily accept the belief that Captain M'Quhae and his officers had mistaken a sea-elephant for a creature such as they describe and picture. To begin with, although it might be probable enough that no one on board the *Dædalus* had ever seen a gigantic seal freely swimming in the open ocean—a sight which Professor Owen himself had certainly never seen—yet we can hardly suppose they would not have known a sea-elephant under such circumstances. Even if they had never seen a sea-elephant at all, they would surely know what such an animal is like. No one could mistake a sea-elephant for any other living creature, even though his acquaintance with the animal were limited to museum specimens or pictures in books. The supposition that the entire animal, that is, its entire length, should be mistaken for 30 or 40 feet of the length of a serpentine neck,

seems, in my judgment, as startling as the ingenious theory thrown out by some naturalists when they first heard of the giraffe—to the effect that some one of lively imagination had mistaken the entire body of a short-horned antelope for the neck of a much larger animal !

Captain M'Quhae immediately replied :—“ I assert that neither was it a common seal nor a sea-elephant ; its great length and its totally different physiognomy precluding the possibility of its being a *Phoca* of any species. The head was flat, and not a capacious vaulted cranium ; nor had it a stiff, inflexible trunk—a conclusion to which Professor Owen has jumped, most certainly not justified by my simple statement, that ‘ no portion of the 60 feet seen by us was used in propelling it through the water, either by vertical or horizontal undulation.’ ” He explained that the calculation of the creature’s length was made before, not after, the idea had been entertained that the animal was a serpent, and that he and his officers were “ too well accustomed to judge of lengths and breadths of objects in the sea to mistake a real substance and an actual living body, coolly and dispassionately contemplated, at so short a distance too, for the ‘ eddy caused by the action of the deeply immersed fins and tail of a rapidly moving, gigantic seal raising its head above the water,’ as Professor Owen imagines, in quest of its lost iceberg.” He next disposed of Owen’s assertion that the idea of clothing the serpent with a mane had been suggested by old Pontoppidan’s story, simply because he had never seen Pontoppidan’s account or heard of Pontoppidan’s sea-serpent, until he had told his own tale in London. Finally, he added, “ I deny the existence of excitement, or the possibility of optical illusion. I adhere to the statement as to form, colour, and dimensions, contained in my report to the Admiralty.”

A narrative which appeared in the *Times* early in 1849 must be referred to in this place, as not being readily explicable by Professor Owen’s hypothesis. It was written by Mr. R. Davidson, superintending surgeon, Najpore Subsidiary

Force, Kamptee, and was to the following effect (I abridge it considerably):—When at a considerable distance southwest of the Cape of Good Hope, Mr. Davidson, Captain Petrie, of the *Royal Saxon*, a steerage passenger, and the man at the wheel, saw “an animal of which no more correct description could be given than that by Captain M‘Quhæ. It passed within 35 yards of the ship, without altering its course in the least; but as it came right abreast of us it slowly turned its head towards us.” About one-third of the upper part of its body was above water, “in nearly its whole length; and we could see the water curling up on its breast as it moved along, but by what means it moved we could not perceive.” They *saw this creature in its whole length* with the exception of a small portion of the tail which was under water; and by comparing its length with that of the *Royal Saxon*, 600 feet, when exactly alongside in passing, they calculated it to be in length as well as in other dimensions greater than the animal described by Captain M‘Quhæ.

In the year 1852 two statements were made, one by Captain Steele, 9th Lancers, the other by one of the officers of the ship *Barham* (India merchantman), to the effect that an animal of a serpentine appearance had been seen about 500 yards from that ship (in longitude 40° E. and 37° 16' S., that is, east of the south-eastern corner of Africa). “We saw him,” said the former, “about 16 or 20 feet out of the water, and he *spouted* a long way from his head”—that is, I suppose, he spouted to some distance, not, as the words really imply, at a part of his neck far removed from the head. Down his back he had a crest like a cock’s comb, and was going very slowly through the water, but left a wake of about 50 or 60 feet, as if dragging a long body after him. The captain put the ship off her course to run down to him, but as we approached him he went down. His colour was green with light spots. He was seen by every one on board.” The other witness gives a similar account, adding that the creature kept moving his head up and down, and was surrounded by hundreds of birds. “We at first thought it

was a dead whale. . . . When we were within 100 yards he slowly sank into the depths of the sea ; while we were at dinner he was seen again." Mr. Alfred Newton, the well-known naturalist, guarantees his personal acquaintance with one of the recipients of the letters just quoted from. But such a guarantee is, of course, no sufficient guarantee of the authenticity of the narrative. Even if the narrative be accepted, the case seems a very doubtful one. The birds form a suspicious element in the story. Why should birds cluster around a living sea creature? It seems to me probable that the sea-weed theory, presently to be noticed, gives the best explanation of this case. Possibly some great aggregation of sea-weed was there, in which were entangled divers objects desirable to birds and to fishes. These last may have dragged the mass under water when the ship approached, being perhaps more or less entangled in it—and it floated up again afterwards. The spouting may have been simply the play of water over the part mistaken for the head.

The sea-weed theory of the sea-serpent was broached in February, 1849, and supported by a narrative not unlike the last. When the British ship *Brazilian* was becalmed almost exactly in the spot where M'Quhæ had seen his monster, Mr. Herriman, the commander, perceived something right abeam, about half a mile to the westward, "stretched along the water to the length of about 25 or 30 feet, and perceptibly moving from the ship with a steady, sinuous motion. The head, which seemed to be lifted several feet above the waters, had something resembling a mane, running down to the floating portion, and within about 6 feet of the tail it forked out into a sort of double fin." Mr. Herriman, his first mate, Mr. Long, and several of the passengers, after surveying the object for some time, came to the unanimous conclusion that it must be the sea-serpent seen by Captain M'Quhæ. "As the *Brazilian* was making no headway, Mr. Herriman, determining to bring all doubts to an issue, had a boat lowered down, and taking two hands on board,

together with Mr. Boyd, of Peterhead, near Aberdeen, one of the passengers, who acted as steersman under the direction of the captain, they approached the monster, Captain Herriman standing on the bow of the boat, armed with a harpoon to commence the onslaught. The combat, however, was not attended with the danger which those on board apprehended ; for on coming close to the object it was found to be nothing more than an immense piece of sea-weed, evidently detached from a coral reef and drifting with the current, which sets constantly to the westward in this latitude, and which, together with the swell left by the subsidence of the gale, gave it the sinuous, snake-like motion."

A statement was published by Captain Harrington in the *Times* of February, 1858, to the effect that from his ship *Castilian*, then distant ten miles from the north-east end of St. Helena, he and his officers had seen a huge marine animal within 20 yards of the ship ; that it disappeared for about half a minute, and then made its appearance in the same manner again, showing distinctly its neck and head about 10 or 12 feet out of the water. "Its head was shaped like a long nun-buoy," proceeds Captain Harrington, "and I suppose the diameter to have been 7 or 8 feet in the largest part, with a kind of scroll, or tuft, of loose skin encircling it about 2 feet from the top ; the water was discoloured for several hundred feet from its head. . . . From what we saw from the deck, we conclude that it must have been over 200 feet long. The boatswain and several of the crew who observed it from the top-gallant forecastle,\* (query, cross-trees?) state that it was more than double the length of the ship, in which case it must have been 500 feet. Be that as it may, I am convinced that it belonged to the serpent tribe ; it was of a dark colour about the head, and was covered with several white spots."

This immediately called out a statement from Captain F.

\* This nautical expression is new to me. Top-gallants—fore, main, and mizen—I know, and forecastle I know, but the top-gallant forecastle I do not know.

Smith, of the ship *Pekin*, that on December 28, not far from the place where the *Dædalus* had encountered the supposed sea-serpent, he had seen, at a distance of about half a mile, a creature which was declared by all hands to be the great sea-serpent, but proved eventually to be a piece of gigantic sea-weed. "I have no doubt," he says, that the great sea-serpent seen from the *Dædalus* "was a piece of the same weed."

It will have been noticed that the sea-weed sea-serpents, seen by Captain F. Smith and by Captain Herriman, were both at a distance of half a mile, at which distance one can readily understand that a piece of sea-weed might be mistaken for a living creature. This is rather different from the case of the *Dædalus* sea-serpent, which passed so near that had it been a man of the captain's acquaintance he could have recognized that man's features with the naked eye. The case, too, of Captain Harrington's sea-serpent, seen within 20 yards of the *Castilian*, can hardly be compared to those cases in which sea-weed, more than 800 yards from the ship, was mistaken for a living animal. An officer of the *Dædalus* thus disposed of Captain Smith's imputation :— "The object seen from the ship was beyond all question a living animal, moving rapidly through the water against a cross sea, and within five points of a fresh breeze, with such velocity that the water was surging against its chest as it passed along at a rate probably of ten miles per hour. Captain M'Quhae's first impulse was to tack in pursuit, but he reflected that we could neither lay up for it nor overhaul it in speed. There was nothing to be done, therefore, but to observe it as accurately as we could with our glasses as it came up under our lee quarter and passed away to windward, being at its nearest position not more than 200 yards from us; *the eye, the mouth, the nostril, the colour, and the form, all being most distinctly visible to us.* . . . My impression was that it was rather of a lizard than a serpentine character, as its movement was steady and uniform, *as if propelled by fins*, not by any undulatory power."

But all the evidence heretofore obtained respecting the sea-serpent, although regarded by many naturalists, Gosse, Newman, Wilson, and others, as demonstrating the existence of some as yet unclassified monster of the deep, seems altogether indecisive by comparison with that which has recently been given by the captain, mates, and crew of the ship *Pauline*. In this case, assuredly, we have not to deal with a mass of sea-weed, the floating trunk of a tree, a sea-elephant hastening to his home amid the icebergs, or with any of the other more or less ingenious explanations of observations previously made. We have either the case of an actual living animal, monstrous, fierce, and carnivorous, or else the five men who deposed on oath to the stated facts devised the story between them, and wilfully perjured themselves for no conceivable purpose—that, too, not as men have been known to perjure themselves under the belief that none could know of their infamy, but with the certainty on the part of each that four others (any one of whom might one day shame him and the rest by confessing) knew the real facts of the case.

The story of the *Pauline* sea-serpent ran simply as follows, as attested at the Liverpool police-court:—"We, the undersigned, captain, officers, and crew of the bark *Pauline*, of London, do solemnly and sincerely declare, that on July 8, 1875, in latitude  $5^{\circ} 13' S.$ , longitude  $35^{\circ} W.$ , we observed three large sperm whales, and one of them was gripped round the body with two turns of what appeared to be a huge serpent. The head and tail appeared to have a length beyond the coils of about 30 feet, and its girth 8 or 9 feet. The serpent whirled its victim round and round for about fifteen minutes, and then suddenly dragged the whale to the bottom, head first.—George Drevat, master; Horatio Thompson, chief mate; John H. Landells, second mate; William Lewarn, steward; Owen Baker, A.B. Again on the 13th July a similar serpent was seen about 200 yards off, shooting itself along the surface, head and neck being out of the water several feet. This was seen only by the captain

and an ordinary seaman.—George Drevat. A few moments afterwards it was seen elevated some 60 feet perpendicularly in the air by the chief officer and two seamen, whose signatures are affixed.—Horatio Thompson, Owen Baker, William Lewarn."

The usual length of the cachalot or sperm whale is about 70 feet, and its girth about 50 feet. If we assign to the unfortunate whale which was captured on this occasion, a length of only 50 feet, and a girth of only 35 feet, we should still have for the entire length of the supposed serpent about 100 feet. This can hardly exceed the truth, since the three whales are called large sperm whales. With a length of 100 feet and a girth of about 9 feet, however, a serpent would have no chance in an attempt to capture a sperm whale 50 feet long and 35 feet in girth, for the simple reason that the whale would be a good deal heavier than its opponent. In a contest in open sea, where one animal seeks to capture another bodily, weight is all-important. We can hardly suppose the whale could be so compassed by the coils of his enemy as to be rendered powerless; in fact, the contest lasted fifteen minutes, during the whole of which time the so-called serpent was whirling its victim round, though more massive than itself, through the water. On the whole, it seems reasonable to conclude—in fact, the opinion is almost forced upon us—that besides the serpentine portion of its bulk, which was revealed to view, the creature, thus whirling round a large sperm whale, had a massive concealed body, provided with propelling paddles of enormous power. These were at work all the time the struggle went on, enabling the creature to whirl round its enemy easily, whereas a serpentine form, with two-thirds of its length, at least, coiled close round another body, would have had no propulsive power left, or very little, in the remaining 30 feet of its length, including both the head and tail ends beyond the coils. Such a creature as an *enaliosaurus* could no doubt have done what a serpent of twice the supposed length would have attempted in vain—viz., dagged down into the depths of the sea the mighty bulk of a cachalot whale.

When all the evidence is carefully weighed, we appear led to the conclusion that at least one large marine animal exists which has not as yet been classified among the known species of the present era. It would appear that this animal has certainly a serpentine neck, and a head small compared with its body, but large compared with the diameter of the neck. It is probably an air-breather and warm-blooded, and certainly carnivorous. Its propulsive power is great and apparently independent of undulations of its body, wherefore it presumably has powerful concealed paddles. All these circumstances correspond with the belief that it is a modern representative of the long-neck plesiosaurians of the great secondary or mesozoic era, a member of that strange family of animals whose figure has been compared to that which would be formed by drawing a serpent through the body of a sea-turtle.

Against this view sundry objections have been raised, which must now be briefly considered.

In the first place, Professor Owen pointed out that the sea-saurians of the secondary period have been replaced in the tertiary and present seas by the whales and allied races. No whales are found in the secondary strata, no saurians in the tertiary. "It seems to me less probable," he says, "that no part of the carcase of such reptiles should have ever been discovered in a recent unfossilized state, than that men should have been deceived by a cursory view of a partly submerged and rapidly moving animal which might only be strange to themselves. In other words, I regard the negative evidence from the utter absence of any of the recent remains of great sea-serpents, krakens, or enaliosauria, as stronger against their actual existence, than the positive statements which have hitherto weighed with the public mind in favour of their existence. A larger body of evidence from eye-witnesses might be got together in proof of ghosts than of the sea-serpent."

To this it has been replied that genera are now known to exist, as the *Chimæra*, the long-necked river tortoise, and

the iguana, which are closely related to forms which existed in the secondary era, while no traces have been found of them in any of the intermediate or tertiary strata. The chimæra is a case precisely analogous to the supposed case of the enaliosaurus, for the chimaera is but rarely seen, like the supposed enaliosaurus, is found in the same and absent from the same fossiliferous strata. Agassiz is quoted in the *Zoologist*, page 2395, as saying that it would be in precise conformity with analogy that such an animal as the enaliosaurus should exist in the American seas, as he had found numerous instances in which the fossil forms of the Old World were represented by living types in the New. In close conformity with this opinion is a statement made by Captain the Hon. George Hope, that when in the British ship *Fly*, in the Gulf of California, the sea being perfectly calm and transparent, he saw at the bottom a large marine animal, with the head and general figure of an alligator, but the neck much longer, and with four large paddles instead of legs. Here, then, unless this officer was altogether deceived, which seems quite unlikely under the circumstances, was a veritable enaliosaurus, though of a far smaller species, probably, than the creature mistaken for a sea-serpent.

As for the absence of remains, Mr. Darwin has pointed out that the fossils we possess are but fragments accidentally preserved by favouring circumstances in an almost total wreck. We have many instances of existent creatures, even such as would have a far better chance of floating after death, and so getting stranded where their bones might be found, which have left no trace of their existence. A whale possessing two dorsal fins was said to have been seen by Smaltz, a Sicilian naturalist; but the statement was rejected, until a shoal of these whales were seen by two eminent French zoologists, MM. Quoy and Gaimard. No carcase, skeleton, or bone of this whale has ever been discovered. For seventeen hours a ship, in which Mr. Gosse was travelling to Jamaica, was surrounded by a species of whale never before noticed—30 feet long, black above and

white beneath, with swimming paws white on the upper surface. Here, he says, was "a whale of large size, occurring in great numbers in the North Atlantic, which on no other occasion has fallen under scientific observation. The toothless whale of Havre, a species actually inhabiting the British Channel, is only known from a single specimen accidentally stranded on the French coast; and another whale, also British, is known only from a single specimen cast ashore on the Elgin coast, and there seen and described by the naturalist Sowerby."

Dr. Andrew Wilson, in an interesting paper, in which he maintains that sea-serpent tales are not to be treated with derision, but are worthy of serious consideration, "supported as they are by zoological science, and in the actual details of the case by evidence as trustworthy in many cases as that received in our courts of law," expresses the opinion that plesiosauri and ichthyosauri have been unnecessarily disinterred to do duty for the sea-serpents. But he offers as an alternative only the ribbon-fish; and though some of these may attain enormous dimensions, yet we have seen that some of the accounts of the supposed sea-serpent, and especially the latest narrative by the captain and crew of the *Pauline*, cannot possibly be explained by any creature so flat and relatively so feeble as the ribbon-fish.

On the whole, it appears to me that a very strong case has been made out for the enaliosaurian, or serpent-turtle, theory of the so-called sea-serpent.

One of the ribbon-fish mentioned by Dr. Wilson, which was captured, and measured more than 60 feet in length, might however fairly take its place among strange sea creatures. I scarcely know whether to add to the number a monstrous animal like a tadpole, or even more perhaps like a gigantic skate, 200 feet in length, said to have been seen in the Malacca Straits by Captain Webster and Surgeon Anderson, of the ship *Nestor*. Perhaps, indeed, this monster, mistaken in the first instance for a shoal, but presently found to be travelling along at the rate of about ten knots an hour,

better deserves to be called a strange sea creature even than any of those which have been dealt with in the preceding pages. But the only account I have yet seen of Captain Webster's statement, and Mr. Anderson's corroboration, appeared in an American newspaper ; and though the story is exceedingly well authenticated if the newspaper account of the matter is true, it would not be at all a new feature in American journalism if not only the story itself, but all the alleged circumstances of its narration, should in the long run prove to be pure invention.

## *ON SOME MARVELS IN TELEGRAPHY.*

WITHIN the last few years Electric Telegraphy has received some developments which seem wonderful even by comparison with those other wonders which had before been achieved by this method of communication. In reality, all the marvels of electric telegraphy are involved, so to speak, in the great marvel of electricity itself, a phenomenon as yet utterly beyond the interpretation of physicists, though not more so than its fellow marvels, light and heat. We may, indeed, draw a comparison between some of the most wonderful results which have recently been achieved by the study of heat and light and those effected in the application of electricity to telegraphy. It is as startling to those unfamiliar with the characteristics of light, or rather with certain peculiarities resulting from these characteristics, to be told that an astronomer can tell whether there is water in the air of Mars or Venus, or iron vapour in the atmosphere of Aldebaran or Betelgeux, as it is to those unfamiliar with the characteristics of electricity, or with the results obtained in consequence of these characteristics, to be told that a written message can be copied by telegraph, a map or diagram reproduced, or, most wonderful of all, a musical air correctly repeated, or a verbal message made verbally audible. Telegraphic marvels such as these bear to the original

marvel of mere telegraphic communication, somewhat the same relation which the marvels of spectroscopic analysis as applied to the celestial orbs bear to that older marvel, the telescopic scrutiny of those bodies. In each case, also, there lies at the back of all these marvels a greater marvel yet—electricity in the one case, light in the other.

I propose in this essay to sketch the principles on which some of the more recent wonders of telegraphic communication depend. I do not intend to describe at any length the actual details or construction of the various instruments employed. Precisely as the principles of spectroscopic analysis can be made clear to the general reader without the examination of the peculiarities of spectroscopic instruments, so can the methods and principles of telegraphic communication be understood without examining instrumental details. In fact, it may be questioned whether general explanations are not in such cases more useful than more detailed ones, seeing that these must of necessity be insufficient for a student who requires to know the subject practically in all its details, while they deter the general reader by technicalities in which he cannot be expected to take any interest. If it be asked, whether I myself, who undertake to explain the principles of certain methods of telegraphic communication, have examined *practically* the actual instrumental working of these methods, I answer frankly that I have not done so. As some sort of proof, however, that without such practical familiarity with working details the principles of the construction of instruments may be thoroughly understood, I may remind the reader (see p. 96) that the first spectroscopic battery I ever looked through—one in which the dispersive power before obtained in such instruments had been practically doubled—was of my own invention, constructed (with a slight mechanical modification) by Mr. Browning, and applied at once successfully to the study of the sun by Mr. Huggins, in whose observatory I saw through this instrument the solar spectrum extended to a length which, could it all have been seen at once, would have

equalled many feet.\* On the other hand, it is possible to have a considerable practical experience of scientific instruments without sound knowledge of the principles of their construction ; insomuch that instances have been known in which men who have effected important discoveries by the use of some scientific instrument, have afterwards obtained their first clear conception of the principles of its construction from a popular description.

It may be well to consider, though briefly, some of the methods of communication which were employed before the electric telegraph was invented. Some of the methods of electric telegraphy have their antitypes, so to speak, in methods of telegraphy used ages before the application of electricity. The earliest employment of telegraphy was probably in signalling the approach of invading armies by beacon fires. The use of this method must have been well known in the time of Jeremiah, since he warns the Benjamites "to set up a sign of fire in Beth-haccerem," because "evil appeareth out of the north and great destruction." Later, instead of the simple beacon fire, combinations were used. Thus, by an Act of the Scottish Parliament in 1455, the blazing of one bale indicated the probable approach of the English, two bales that they were coming indeed, and four bales blazing beside each other that they were in great force. The smoke of beacon fires served as signals by day, but not so effectively, except under very favourable atmospheric conditions.

Torches held in the hand, waved, depressed, and so forth, were anciently used in military signalling at night ; while in the day-time boards of various figures in different positions indicated either different messages or different letters, as might be pre-arranged.

Hooke communicated to the Royal Society in 1684 a paper describing a method of "communicating one's mind

\* The instrument was lent to Mr. Huggins by Mr. W. Spottiswoode. It has been recently employed successfully at Greenwich.

at great distances." The letters were represented by various combinations of straight lines, which might be agreed upon previously if secrecy were desired, otherwise the same forms might represent constantly the same letters. With four straight planks any letter of this alphabet could be formed as wanted, and being then run out on a framework (resembling a gallows in Hooke's picture), could be seen from a distant station. Two curved beams, combined in various ways, served for arbitrary signals.

Chappe, in 1793, devised an improvement on this in what was called the T telegraph. An upright post supported a cross-bar (the top of the T), at each end of which were the short dependent beams, making the figure a complete Roman capital T. The horizontal bar as first used could be worked by ropes within the telegraph-house, so as to be inclined either to right or left. It thus had three positions. Each dependent beam could be worked (also from within the house) so as to turn upwards, horizontally, or downwards (regarding the top bar of the T as horizontal), thus having also three positions. It is easily seen that, since each position of one short beam could be combined with each position of the other, the two together would present three times three arrangements, or nine in all; and as these nine could be given with the cross-bar in any one of its three positions, there were in all twenty-seven possible positions. M. Chappe used an alphabet of only sixteen letters, so that all messages could readily be communicated by this telegraph. For shorter distances, indeed, and in all later uses of Chappe's telegraph, the short beams could be used in intermediate positions, by which 256 different signals could be formed. Such telegraphs were employed on a line beginning at the Louvre and proceeding by Montmartre to Lisle, by which communications were conveyed from the Committee of Public Welfare to the armies in the Low Countries. Telescopes were used at each station. Barrère stated, in an address to the Convention on August 17, 1794, that the news of the recapture of Lisle had been sent by

this line of communication to Paris in one hour after the French troops had entered that city. Thus the message was conveyed at the rate of more than 120 miles per hour.

Various other devices were suggested and employed during the first half of the present century. The semaphores still used in railway signalling illustrate the general form which most of these methods assumed. An upright, with two arms, each capable of assuming six distinct positions (excluding the upright position), would give forty-eight different signals; thus each would give six signals alone, or twelve for the pair, and each of the six signals of one combined with each of the six signals of the other, would give thirty-six signals, making forty-eight in all. This number suffices to express the letters of the alphabet (twenty-five only are needed), the Arabic numerals, and thirteen arbitrary signals.

The progress of improvement in such methods of signalling promised to be rapid, before the invention of the electric telegraph, or rather, before it was shown how the principle of the electric telegraph could be put practically into operation. We have seen that they were capable of transmitting messages with considerable rapidity, more than twice as fast as we could now send a written message by express train. But they were rough and imperfect. They were all, also, exposed to one serious defect. In thick weather they became useless. Sometimes, at the very time when it was most important that messages should be quickly transmitted, fog interrupted the signalling. Sir J. Barrow relates that during the Peninsular War grave anxiety was occasioned for several hours by the interruption of a message from Plymouth, really intended to convey news of a victory. The words transmitted were, "Wellington defeated;" the message of which these words formed the beginning was: "Wellington defeated the French at," etc. As Barrow remarks, if the message had run, "French defeated at," etc., the interruption of the message would have been of less consequence.

Although the employment of electricity as a means of communicating at a distance was suggested before the end of the last century, in fact, so far back as 1774, the idea has only been worked out during the last forty-two years. It is curious indeed to note that until the middle of the present century the word "telegraph," which is now always understood as equivalent to electric telegraph, unless the contrary is expressed, was commonly understood to refer to semaphore signalling,\* unless the word "electric" were added.

The general principle underlying all systems of telegraphic communication by electricity is very commonly misunderstood. The idea seems to prevail that electricity can be sent out along a wire to any place where some suitable arrangement has been made to receive it. In one sense this is correct. But the fact that the electricity has to make a circuit, returning to the place from which it is transmitted, seems not generally understood. Yet, unless this is understood, the principle, even the possibility, of electric communication is not recognized.

Let us, at the outset, clearly understand the nature of electric communication.

In a variety of ways, a certain property called electricity can be excited in all bodies, but more readily in some than in others. This property presents itself in two forms, which are called positive and negative electricity, words which we may conveniently use, but which must not be regarded as representing any real knowledge of the distinction between these two kinds of electricity. In fact, let it be remembered throughout, that we do not in the least know what elec-

\* Thus in *Christie Johnstone*, written in 1853, when Flucker Johnstone tells Christie the story of the widow's sorrows, giving it word for word, and even throwing in what dramatists call "the business," he says, "'Here ye'll play your hand like a geraffe.' 'Geraffe?' she says; 'that's a beast, I'm thinking.' 'Na; it's the thing on the hill that makes signals.' 'Telegraph, ye fulish goloshen!' 'Oo, ay, telegraph! geraffe's sunnest said for a'.'" "Playing the hand like a telegraph" would now be as unmeaning as Flucker Johnstone's original description.

tricity is ; we only know certain of the phenomena which it produces. Any body which has become charged with electricity, either positive or negative, will part with its charge to bodies in a neutral condition, or charged with the opposite electricity (negative or positive). But the transference is made much more readily to some substances than to others—so slowly, indeed, to some, that in ordinary experiments the transference may be regarded as not taking place at all. Substances of the former kind are called good conductors of electricity ; those which receive the transfer of electricity less readily are said to be bad conductors ; and those which scarcely receive it at all are called insulating substances. The reader must not confound the quality I am here speaking of with readiness to become charged with electricity. On the contrary, the bodies which most freely receive and transmit electricity are least readily charged with electricity, while insulating substances are readily electrified. Glass is an insulator, but if glass is briskly rubbed with silk it becomes charged (or rather, the part rubbed becomes charged) with positive electricity, formerly called *vitreous* electricity for this reason ; and again, if wax or resin, which are both good insulators, be rubbed with cloth or flannel, the part rubbed becomes charged with negative, formerly called *resinous*, electricity.

Electricity, then, positive or negative, however generated, passes freely along conducting substances, but is stopped by an insulating body, just as light passes through transparent substances, but is stopped by an opaque body. Moreover, electricity may be made to pass to any distance along conducting bodies suitably insulated. Thus, it might seem that we have here the problem of distant communication solved. In fact, the first suggestion of the use of electricity in telegraphy was based on this property. When a charge of electricity has been obtained by the use of an ordinary electrical machine, this charge can be drawn off at a distant point, if a conducting channel properly insulated connects that point with the bodies (of whatever nature)

which have been charged with electricity. In 1747, Dr. Watson exhibited electrical effects from the discharges of Leyden jars (vessels suitably constructed to receive and retain electricity) at a distance of two miles from the electrical machine. In 1774, Le Sage proposed that by means of wires the electricity developed by an electrical machine should be transmitted by insulated wires to a point where an electroscope, or instrument for indicating the presence of electricity, should, by its movements, mark the letters of the alphabet, one wire being provided for each letter. In 1798 Béthencourt repeated Watson's experiment, increasing the distance to twenty-seven miles, the extremities of his line of communication being at Madrid and Aranjuez. (Guillemin, by the way, in his "Applications of the Physical Forces," passes over Watson's experiment; in fact, throughout his chapters on the electric telegraph, the steam engine, and other subjects, he seems desirous of conveying as far as possible the impression that all the great advances of modern science had their origin in Paris and its neighbourhood.)

From Watson's time until 1823 attempts were made in this country and on the Continent to make the electrical machine serve as the means of telegraphic communication. All the familiar phenomena of the lecture-room have been suggested as signals. The motion of pith balls, the electric spark, the perforation of paper by the spark, the discharge of sparks on a fulminating pane (a glass sheet on which pieces of tinfoil are suitably arranged, so that sparks passing from one to another form various figures or devices), and other phenomena, were proposed and employed experimentally. But practically these methods were not effectual. The familiar phenomenon of the electric spark explains the cause of failure. The spark indicates the passage of electricity across an insulating medium—dry air—when a good conductor approaches within a certain distance of the charged body. The greater the charge of electricity, the greater is the distance over which the electricity will thus make its escape. Insulation, then, for many miles of wire, and still

more for a complete system of communication such as we now have, was hopeless, so long as frictional electricity was employed, or considerable electrical intensity required.

We have now to consider how galvanic electricity, discovered in 1790, was rendered available for telegraphic communication. In the first place, let us consider what galvanic or voltaic electricity is.

I have said that electricity can be generated in many ways. It may be said, indeed, that every change in the condition of a substance, whether from mechanical causes, as, for instance, a blow, a series of small blows, friction, and so forth, or from change of temperature, moisture, and the like, or from the action of light, or from chemical processes, results in the development of more or less electricity.

When a plate of metal is placed in a vessel containing some acid (diluted) which acts chemically on the metal, this action generates negative electricity, which passes away as it is generated. But if a plate of a different metal, either not chemically affected by the acid or less affected than the former, be placed in the dilute acid, the two plates being only partially immersed and not in contact, then, when a wire is carried from one plate to the other, the excess of positive electricity in the plate least affected by the acid is conveyed to the other, or, in effect, discharged ; the chemical action, however, continues, or rather is markedly increased, fresh electricity is generated, and the excess of positive electricity in the plate least affected is constantly discharged. Thus, along the wire connecting the two metals a current of electricity passes from the metal least affected to the metal most affected ; a current of negative electricity passes in a contrary direction in the dilute acid.

I have spoken here of currents passing along the wire and in the acid, and shall have occasion hereafter to speak of the plate of metal least affected as the positive pole, this plate being regarded, in this case, as a source whence a current of positive electricity flows along the wire connection to the other plate, which is called the negative pole. But I

must remind the reader that this is only a convenient way of expressing the fact that the wire assumes a certain condition when it connects two such plates, and is capable of producing certain effects. Whether in reality any process is taking place which can be justly compared to the flow of a current one way or the other, or whether a negative current flows along the circuit one way, while the positive current flows the other way, are questions still unanswered. We need not here enter into them, however. In fact, very little is known about these points. Nor need we consider here the various ways in which many pairs of plates such as I have described can be combined in many vessels of dilute acid to strengthen the current. Let it simply be noted that such a combination is called a battery; that when the extreme plates of opposite kinds are connected by a wire, a current of electricity passes along the wire from the extreme plate of that metal which is least affected, forming the positive pole, to the other extreme plate of that metal which is most affected and forms the negative pole. The metals commonly employed are zinc and copper, the former being the one most affected by the action of the dilute acid, usually sulphuric acid. But it must here be mentioned that the chemical process, affecting both metals, but one chiefly, would soon render a battery of the kind described useless; wherefore arrangements are made in various ways for maintaining the efficiency of the dilute acid and of the metallic plates, especially the copper: for the action of the acid on the zinc tends, otherwise, to form on the copper a deposit of zinc. I need not describe the various arrangements for forming what are called constant batteries, as Daniell's, Grove's, Bunsen's, and others. Let it be understood that, instead of a current which would rapidly grow weaker and weaker, these batteries give a steady current for a considerable time. Without this, as will presently be seen, telegraphic communication would be impossible.

We have, then, in a galvanic battery a steady source of electricity. This electricity is of low intensity, incompetent

to produce the more striking phenomena of frictional electricity. Let us, however, consider how it would operate at a distance.

The current will pass along any length of conducting substance properly insulated. Suppose, then, an insulated wire passes from the positive pole of a battery at a station A to a station B, and thence back to the negative pole at the station A. Then the current passes along it, and this can be indicated at B by some action such as electricity of low intensity can produce. If now the continuity of the wire be interrupted close by the positive pole at A, the current ceases and the action is no longer produced. The observer at B knows then that the continuity of the wire has been interrupted; he has been, in fact, signalled to that effect.

But, as I have said, the electrical phenomena which can be produced by the current along a wire connecting the positive and negative poles of a galvanic battery are not striking. They do not afford effective signals when the distance traversed is very great and the battery not exceptionally strong. Thus, at first, galvanic electricity was not more successful in practice than frictional electricity.

It was not until the effect of the galvanic current on the magnetic needle had been discovered that electricity became practically available in telegraphy.

Oersted discovered in 1820 that a magnetic needle poised horizontally is deflected when the galvanic current passes above it (parallel to the needle's length) or below it. If the current passes above it, the north end of the needle turns towards the east when the current travels from north to south, but towards the west when the current travels from south to north; on the other hand, if the current passes below the needle, the north end turns towards the west when the current travels from south to north, and towards the east when the current travels from north to south. The deflection will be greater or less according to the power of the current. It would be very slight indeed in the case of a needle, however

delicately poised, above or below which passed a wire conveying a galvanic current from a distant station. But the effect can be intensified, as follows :—

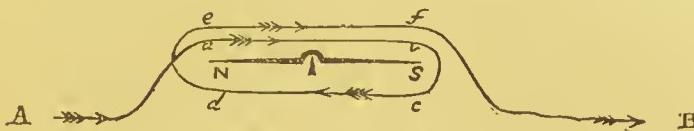


FIG. I.

Suppose  $a b c d e f$  to be a part of the wire from A to B, passing above a delicately poised magnetic needle N.S., along  $a b$  and then below the needle along  $c d$ , and then above again along  $e f$ , and so to the station B. Let a current traverse the wire in the direction shown by the arrows. Then N, the north end of the needle, is deflected towards the east by the current passing along  $a b$ . But it is also deflected to the east by the current passing along  $c d$ ; for this produces a deflection the reverse of that which would be produced by a current in the same direction above the needle—that is, in direction  $b a$ , and therefore the same as that produced by the current along  $a b$ . The current along  $e f$  also, of course, produces a deflection of the end N towards the east. All three parts, then,  $a b$ ,  $c d$ ,  $e f$ , conspire to increase the deflection of the end N towards the east. If the wire were twisted once again round N S, the deflection would be further increased; and finally, if the wire be coiled in the way shown in Fig. 1, but with a great number of coils, the deflection of the north end towards the east, almost imperceptible without such coils, will become sufficiently obvious. If the direction of the current be changed, the end N will be correspondingly deflected towards the west.

The needle need not be suspended horizontally. If it hang vertically, that is, turn freely on a horizontal axis, and the coil be carried round it as above described, the deflection of the upper end will be to the right or to the left, according to the direction of the current. The needle actually seen, moreover, is not the one acted upon by the

current. This needle is inside the coil ; the needle seen turns on the same axis, which projects through the coil.

If, then, the observer at the station B have a magnetic needle suitably suspended, round which the wire from the battery at A has been coiled, he can tell by the movement of the needle whether a current is passing along the wire in one direction or in the other ; while if the needle is at rest he knows that no current is passing.

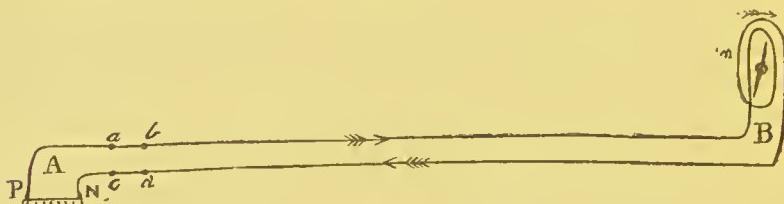


FIG. 2.

Now suppose that P and N, Fig. 2, are the positive and negative poles of a galvanic battery at A, and that a wire passes from P to the station B, where it is coiled round a needle suspended vertically at n, and thence passes to the negative pole N. Let the wire be interrupted at a b and also at c d. Then no current passes along the wire, and the needle n remains at rest in a vertical position. Now suppose the

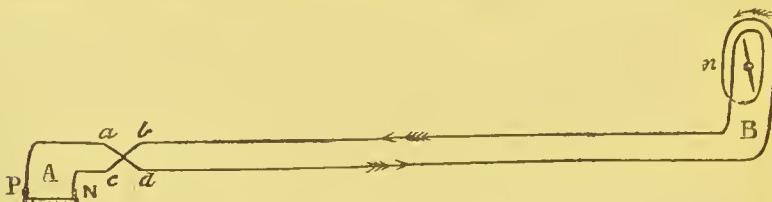


FIG. 3.

points a b connected by the wire a b, and at the same moment the points c d connected by the wire c d, then a current flows along P a b to B, as shown in Fig. 2, circuiting the coil round the needle n and returning by d c to N. The upper end of the needle is deflected to the right while this current continues to flow ; returning to rest when the connection is broken at a b and c d. Next, let c b and a d be simultane-

ously connected as shown by the cross-lines in Fig. 3. (It will be understood that  $a\ d$  and  $b\ c$  do not touch each other where they cross.) The current will now flow from  $P$  along  $a\ d$  to  $B$ , circuiting round the needle  $n$  in a contrary direction to that in which it flowed in the former case, returning by  $b\ c$  to  $N$ . The upper end of the needle is deflected then to the left while the current continues to flow along this course.

I need not here describe the mechanical devices by which the connection at  $a\ b$  and  $c\ d$  can be instantly changed so that the current may flow either along  $a\ b$  and  $d\ c$ , as in Fig. 2, circuiting the needle in one direction, or along  $a\ d$  and  $b\ c$ , as in Fig. 3, circuiting the needle in the other direction. As I said at the outset, this paper is not intended to deal with details of construction, only to describe the general principles of telegraphic communication, and especially those points which have to be explained in order that recent inventions may be understood. The reader will see that nothing can be easier than so to arrange matters that, by turning a handle, either (1),  $a\ b$  and  $c\ d$  may be connected, or, (2),  $a\ d$  and  $c\ b$ , or, (3), both lines of communication interrupted. The mechanism for effecting this is called a *commutator*.

Two points remain, however, to be explained : First, A must be a receiving station as well as a transmitting station ; secondly, the wire connecting  $B$  with  $N$ , in Figs. 2 and 3, can be dispensed with, for it is found that if at  $B$  the wire is carried down to a large metal plate placed some depth underground, while the wire at  $c$  is carried down to another plate similarly buried underground, the circuit is completed even better than along such a return wire as is shown in the figures. The earth either acts the part of a return wire, or else, by continually carrying off the electricity, allows the current to flow continuously along the single wire. We may compare the current carried along the complete wire circuit, to water circulating in a pipe extending continuously from a reservoir to a distance and back again to the reservoir. Water sucked up continuously at one end could be carried through the pipe so long as it was continu-

ously returned to the reservoir at the other; but it could equally be carried through a pipe extending from that reservoir to some place where it could communicate with the open sea—the reservoir itself communicating with the open sea—an arrangement corresponding to that by which the return wire is dispensed with, and the current from the wire received into the earth.

The discovery that the return wire may be dispensed with was made by Steinheil in 1837.

The actual arrangement, then, is in essentials that represented in Fig. 4.

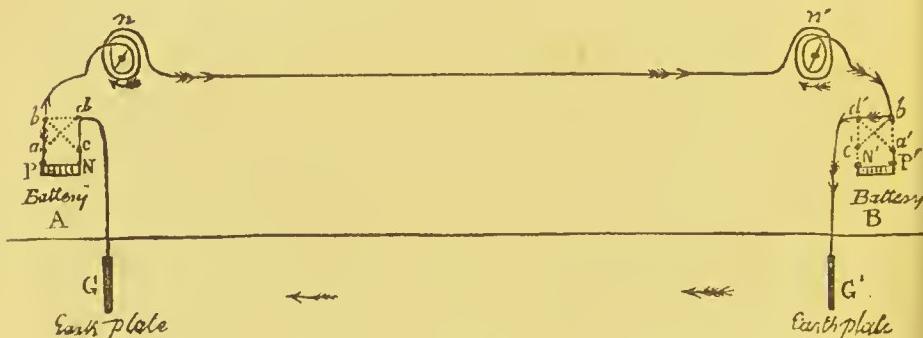


FIG. 4.

A and B are the two stations ; P N is the battery at A, P' N' the battery at B ; P' P' are the positive poles, N' N', the negative poles. At n is the needle of station A, at n' the needle of station B. When the handle of the commutator is in its mean position—which is supposed to be the case at station B—the points b' d' are connected with each other, but neither with a' nor c' ; no current, then, passes from B to A, but station B is in a condition to receive messages. (If b' and d' were not connected, of course no messages could be received, since the current from A would be stopped at b'—which does not mean that it would pass round n' to b', but that, the passage being stopped at b', the current would not flow at all.) When (the commutator at B being in its mean position, or d' b' connected, and communication with c' and a' interrupted) the handle of the commutator at A is turned

from its mean position in *one* direction, *a* and *b* are connected, as are *c* and *d*—as shown in the figure—while the connection between *b* and *d* is broken. Thus the current passes from *P* by *a* and *b*, round the needle *n*; thence to station *B*, round needle *n'*, and by *b'* and *d'*, to the earth plate *c'*; and so along the earth to *G*, and by *d* *c*, to the negative pole *N*. The upper end of the needle of both stations is deflected to the right by the passage of the current in this direction. When the handle of the commutator at *A* is turned in the other direction, *b* and *c* are connected, as also *a* and *d*; the current from *P* passes along *a d* to the ground plate *G*, thence to *c'*, along *d' b'*, round the needle *n'*, back by the wire to the station *A*, where, after circuiting the needle *n* in the same direction as the needle *n'*, it travels by *b* and *c* to the negative pole *N*. The upper end of the needle, at both stations, is deflected to the left by the passage of the current in this direction.

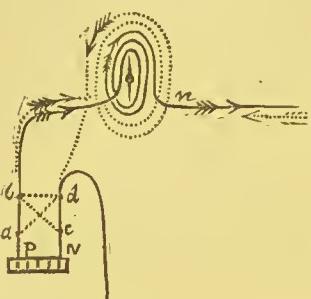
It is easily seen that, with two wires and one battery, two needles can be worked at both stations, either one moving alone, or the other alone, or both together; but for the two to move differently, two batteries must be used. The systems by which either the movements of a single needle, or of a pair of needles, may be made to indicate the various letters of the alphabet, numerals, and so on, need not here be described. They are of course altogether arbitrary, except only that the more frequent occurrence of certain letters, as *e*, *t*, *a*, renders it desirable that these should be represented by the simplest symbols (as by a single deflection to right or left), while letters which occur seldom may require several deflections.

One of the inventions to which the title of this paper relates can now be understood.

In the arrangement described, when a message is transmitted, the needle of the sender vibrates synchronously with the needle at the station to which the message is sent. Therefore, till that message is finished, none can be received at the transmitting station. In what is called duplex tele-

graphy, this state of things is altered, the needle at the sending station being left unaffected by the transmitted current, so as to be able to receive messages, and in self-recording systems to record them. This is done by dividing the current from the battery into two parts of equal efficiency, acting on the needle at the transmitting station in contrary directions, so that this needle remains unaffected,

and ready to indicate signals from the distant station. The principle of this arrangement is indicated in Fig. 5. Here  $a b n$  represents the main wire of communication with the distant station, coiled round the needle of the transmitting station in one direction; the dotted lines indicate a finer short wire, coiled round the needle in a contrary direction. When a message



is transmitted, the current along the main wire tends to deflect the needle at  $n$  in one direction, while the current along the auxiliary wire tends to deflect it in the other direction. If the thickness and length of the short wire are such as to make these two tendencies equal, the needle remains at rest, while a message is transmitted to the distant station along the main wire. In this state of things, if a current is sent from the distant station along the wire in the direction indicated by the dotted arrow, this current also circuits the auxiliary wire, but in the direction indicated by the arrows on the dotted curve, which is the same direction in which it circuits the main wire. Thus the needle is deflected, and a signal received. When the direction of the chief current at the transmitting station is reversed, so also is the direction of the artificial current, so that again the needle is balanced. Similarly, if the direction of the current from the distant station is reversed, so also is the direction in which this current traverses the auxiliary wire, so that again both effects conspire to deflect the needle.

FIG. 5.

There is, however, another way in which an auxiliary wire may be made to work. It may be so arranged that, when a message is transmitted, the divided current flowing equally in opposite directions, the instrument at the sending station is not affected ; but that when the operator at the distant station sends a current along the main wire, this neutralizes the current coming towards him, which current had before balanced the artificial current. The latter, being no longer counterbalanced, deflects the needle ; so that, in point of fact, by this arrangement, the signal received at a station is produced by the artificial current at that station, though of course the real cause of the signal is the transmission of the neutralizing current from the distant station.

The great value of duplex telegraphy is manifest. Not only can messages be sent simultaneously in both directions along the wire—a circumstance which of itself would double the work which the wire is capable of doing—but all loss of time in arranging about the order of outward and homeward messages is prevented. The saving of time is especially important on long lines, and in submarine telegraphy. It is also here that the chief difficulties of duplex telegraphy have been encountered. The chief current and the artificial current must exactly balance each other. For this purpose the flow along each must be equal. In passing through the long wire, the current has to encounter a greater resistance than in traversing the short wire ; to compensate for this difference, the short wire must be much finer than the long one. The longer the main wire, the more delicate is the task of effecting an exact balance. But in the case of submarine wires, another and a much more serious difficulty has to be overcome. A land wire is well insulated. A submarine wire is separated by but a relatively moderate thickness of gutta-percha from water, an excellent conductor, communicating directly with the earth, and is, moreover, surrounded by a protecting sheathing of iron wires, laid spirally round the core, within which lies the copper conductor. Such a cable, as Faraday long since showed,

acts precisely as an enormous Leyden jar; or rather, Faraday showed that such a cable, without the wire sheathing, would act when submerged as a Leyden jar, the conducting wire acting as the interior metallic coating of such a jar, the gutta-percha as the glass of the jar (the insulating medium), and the water acting as the exterior metallic coating. Wheatstone showed further that such a cable, with a wire sheathing, would act as a Leyden jar, even though not submerged, the metal sheathing taking the part of the exterior coating of the jar. Now, regarding the cable thus as a condenser, we see that the transmission of a current along it may in effect be compared with the passage of a fluid along a pipe of considerable capacity, into which and from which it is conveyed by pipes of small capacity. There will be a retardation of the flow of water corresponding to the time necessary to fill up the large part of the pipe; the water may indeed begin to flow through as quickly as though there were no enlargement of the bore of the pipe, but the full flow from the further end will be delayed. Just so it is with a current transmitted through a submarine cable. The current travels instantly (or with the velocity of freest electrical transmission) along the entire line; but it does not attain a sufficient intensity to be recognized for some time, nor its full intensity till a still longer interval has elapsed. The more delicate the means of recognizing its flow, the more quickly is the signal received. The time intervals in question are not, indeed, very great. With Thomson's mirror galvanometer, in which the slightest motion of the needle is indicated by a beam of light (reflected from a small mirror moving with the needle), the Atlantic cable conveys its signal from Valentia to Newfoundland in about one second, while with the less sensitive galvanometer before used the time would be rather more than two seconds.

Now, in duplex telegraphy the artificial current must be equal to the chief current in intensity all the time; so that, since in submarine telegraphy the current rises gradually to

its full strength and as gradually subsides, the artificial current must do the same. Reverting to the illustration derived from the flow of water, if we had a small pipe the rapid flow through which was to carry as much water one way as the slow flow through a large pipe was to carry water the other way, then if the large pipe had a widening along one part of its long course the short pipe would require to have a similar widening along the corresponding part of its short course. And to make the illustration perfect, the widenings along the large pipe should be unequal in different parts of the pipe's length; for the capacity of a submarine cable, regarded as a condenser, is different along different parts of its length. What is wanted, then, for a satisfactory system of duplex telegraphy in the case of submarine cables, is an artificial circuit which shall not only correspond as a whole to the long circuit, but shall reproduce at the corresponding parts of its own length all the varieties of capacity existing along various parts of the length of the submarine cable.

Several attempts have been made by electricians to accomplish this result. Let it be noticed that two points have to be considered: the intensity of the current's action, which depends on the resistance it has to overcome in traversing the circuit; and the velocity of transmission, depending on the capacity of various parts of the circuit to condense or collect electricity. Varley, Stearn, and others have endeavoured by various combinations of condensers with resistance coils to meet these two requisites. But the action of artificial circuits thus arranged was not sufficiently uniform. Recently Mr. J. Muirhead, jun., has met the difficulty in the following way (I follow partially the account given in the *Times* of February 3, 1877, which the reader will now have no difficulty in understanding):—He has formed his second circuit by sheets of paper prepared with paraffin, and having upon one side a strip of tinfoil, wound to and fro to represent resistance. Through this the artificial current is conducted. On the other side is a sheet of tin-

foil to represent the sheathing,\* and to correspond to the capacity of the wire. Each sheet of paper thus prepared may be made to represent precisely a given length of cable, having enough tinfoil on one side to furnish the resistance, and on the other to furnish the capacity. A sufficient number of such sheets would exactly represent the cable, and thus the artificial or non-signalling part of the current would be precisely equivalent to the signalling part, so far as its action on the needle at the transmitting station was concerned. "The new plan was first tried on a working scale," says the *Times*, "on the line between Marseilles and Bona; but it has since been brought into operation from Marseilles to Malta, from Suez to Aden, and lastly, from Aden to Bombay. On a recent occasion when there was a break-down upon the Indo-European line, the duplex system rendered essential service, and maintained telegraphic communication which would otherwise have been most seriously interfered with." The invention, we may well believe, "cannot fail to be highly profitable to the proprietors of submarine cables," or to bring about "before long a material reduction in the cost of messages from places beyond the seas."

The next marvel of telegraphy to be described is the transmission of actual facsimiles of writings or drawings. So far as strict sequence of subject-matter is concerned, I ought, perhaps, at this point, to show how duplex telegraphy has been surpassed by a recent invention, enabling three or four or more messages to be simultaneously transmitted telegraphically. But it will be more convenient to consider this wonderful advance after I have described the methods by which facsimiles of handwriting, etc., are transmitted.

\* Not "to represent the gutta-percha," as stated in the *Times* account of Mr. Muirhead's invention. The gutta-percha corresponds to the insulating material of the artificial circuit; viz., the prepared paper through which the current along the tinfoil strips acts inductively on the coating of tinfoil.

Hitherto we have considered the action of the electric current in deflecting a magnetic needle to right or left, a method of communication leaving no trace of its transmission. We have now to consider a method at once simpler in principle and affording means whereby a permanent record can be left of each message transmitted.

If the insulated wire is twisted in the form of a helix or coil round a bar of soft iron, the bar becomes magnetized while the current is passing. If the bar be bent into the horse-shoe form, as in Fig. 6, where A C B represents the bar,  $a b c d e f$  the coil of insulated wire, the bar acts as a magnet while the current is passing along the coil, but ceases to do so as soon as the current is interrupted.\* If, then, we have a telegraphic wire from a distant station in electric connection with the wire  $a b c$ , the part  $e f$  descending to an earth-plate, then, according as the operator at that distant station transmits or stops the current, the iron A C B is magnetized or demagnetized. The part c is commonly replaced by a flat piece of iron, as is supposed to be the case with the temporary magnets shown in Fig. 7, where this flat piece is below the coils.

So far back as 1838 this property was applied by Morse in America in the recording instrument which bears his name, and is now (with slight modifications) in general use not only in America but on the Continent. The principle

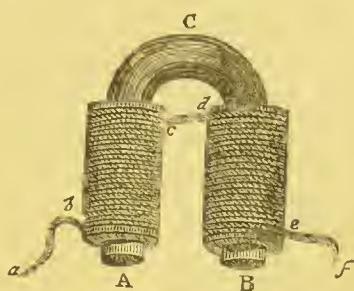


FIG. 6.

\* I must caution the reader against Fig. 348 in Guillemin's *Application of the Physical Forces*, in which the part  $c d$  of the wire is not shown. The two coils are in reality part of a single coil, divided into two to permit of the bar being bent; and to remove the part  $c d$  is to divide the wire, and, of course, break the current. It will be seen that  $c d$  passes from the remote side of coil  $b c$ , Fig. 6, to the near side of coil  $d e$ . If it were taken round the remote side of the latter coil, the current along this would neutralize the effect of the current along the other.

of this instrument is exceedingly simple. Its essential parts are shown in Fig. 7 ; H is the handle, H' the lever of the manipulator at the station A. The manipulator is shown in the position for receiving a message from the station B along the wire w. The handle H' of the manipulator at the station B is shown depressed, making connection at a' with the wire from the battery N' P'. Thus a current passes through the handle to c', along the wire to c and through b to the coil of the temporary magnet M, after circling which it passes to the earth at e and so by E' to the negative pole N'. The passage of this current magnetizes M, which draws down the armature m. Thus the lever l, pulled down on this side, presses up-

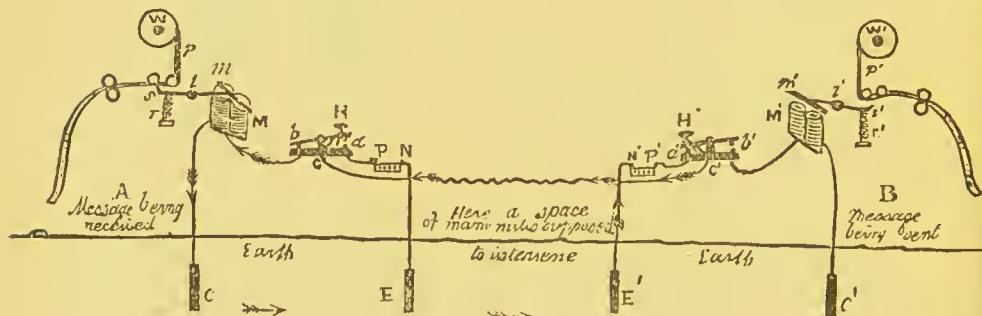


FIG. 7.

wards the pointed style s against a strip of paper p which is steadily rolled off from the wheel w so long as a message is being received. (The mechanism for this purpose is not indicated in Fig. 7.) Thus, so long as the operator at B holds down the handle H', the style s marks the moving strip of paper, the spring r, under the lever s l, drawing the style away so soon as the current ceases to flow and the magnet to act. If he simply depresses the handle for an instant, a dot is marked ; if longer, a dash ; and by various combinations of dots and dashes all the letters, numerals, etc., are indicated. When the operator at B has completed his message, the handle H' being raised by the spring under it (to the position in which H is shown, a message can be received at B.

I have in the figure and description assumed that the current from either station acts directly on the magnet which works the recording style. Usually, in long-distance telegraphy, the current is too weak for this, and the magnet on which it acts is used only to complete the circuit of a local battery, the current from which does the real work of magnetizing M at A or M' at B, as the case may be. A local battery thus employed is called a *relay*.

The Morse instrument will serve to illustrate the *principle* of the methods by which facsimiles are obtained. The details of construction are altogether different from those of the Morse instrument; they also vary greatly in different instruments, and are too complex to be conveniently described here. But the principle, which is the essential point, can be readily understood.

In working the Morse instrument, the operator at B depresses the handle H'. Suppose that this handle is kept depressed by a spring, and that a long strip of paper passing uniformly between the two points at  $\alpha$  prevents contact. Then no current can pass. But if there is a hole in this paper, then when the hole reaches  $\alpha$  the two metal points at  $\alpha$  meet and the current passes. We have here the principle of the Bain telegraph. A long strip of paper is punched with round and long holes, corresponding to the dots and marks of a message by the Morse alphabet. As it passes between a metal wheel and a spring, both forming part of the circuit, it breaks the circuit until a hole allows the spring to touch the wheel, either for a short or longer time-interval, during which the current passes to the other station, where it sets a relay at work. In Bain's system the message is received on a chemically prepared strip of paper, moving uniformly at the receiving station, and connected with the negative pole of the relay battery. When contact is made, the face of the paper is touched by a steel pointer connected with the positive pole, and the current which passes from the end of the pointer through

the paper to the negative pole produces a blue mark on the chemically prepared paper.\*

We see that by Bain's arrangement a paper is marked with dots and lines, corresponding to round and elongated holes, in a ribbon of paper. It is only a step from this to the production of facsimiles of writings or drawings.

Suppose a sheet of paper so prepared as to be a conductor of electricity, and that a message is written on the paper with some non-conducting substance for ink. If that sheet were passed between the knobs at  $\alpha$  (the handle  $H$  being pressed down by a spring), whilst simultaneously a sheet of Bain's chemically prepared paper were passed athwart the steel pointer at the receiving station, there would be traced across the last-named paper a blue line, which would be broken at parts corresponding to those on the other paper where the non-conducting ink interrupted the current. Suppose the process repeated, each paper being slightly shifted so that the line traced across either would be parallel and very close to the former, but precisely corresponding as respects the position of its length. Then this line, also, on the recording paper will be broken at parts corresponding to those in which the line across the transmitting paper meets the writing. If line after line be drawn in this way till the entire breadth of the transmitting paper has been crossed by close parallel lines, the entire breadth of the receiving paper will be covered by closely marked blue lines except where the writing has broken the contact. Thus a negative facsimile of the writing will be found in the manner indicated in Figs. 8 and 9.† In reality, in processes of this kind, the papers (unlike the ribbons on Bain's telegraph) are not carried across in the way I have imagined, but are swept by

\* The paper is soaked in dilute ferrocyanide of potassium, and the passage of the current forms a Prussian blue.

† Sir W. Thomson states, in his altogether excellent article on the electric telegraph, in Nichol's *Cyclopædia*, that the invention of this process is due to Mr. Bakewell.

successive strokes of a movable pointer, along which the current flows ; but the principle is the same.

It is essential, in such a process as I have described, first, that the recording sheet should be carried athwart the pointer which conveys the marking current (or the pointer carried across the recording sheet) in precise accordance with the motion of the transmitting sheet athwart the wire or style which conveys the current to the long wire between the stations (or of this style across the transmitting sheet). The recording sheet and the transmitting sheet must also

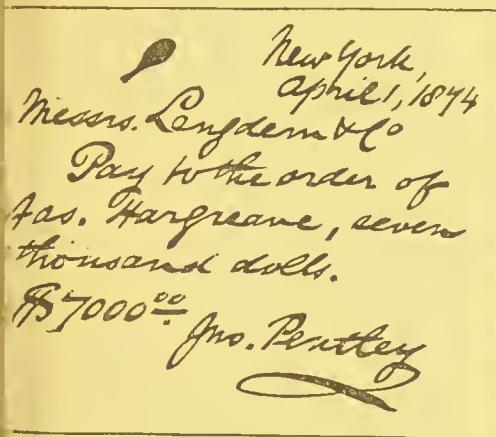


FIG. 8.

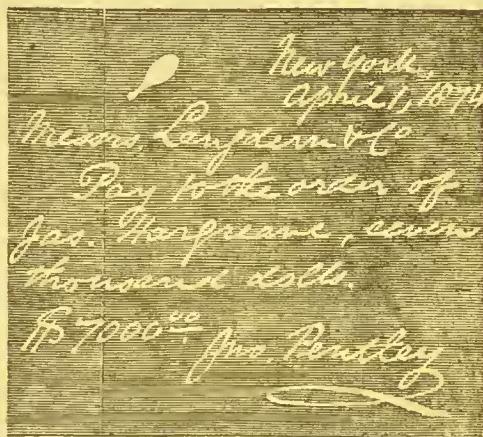


FIG. 9.

be shifted between each stroke by an equal amount. The latter point is easily secured ; the former is secured by causing the mechanism which gives the transmitting style its successive strokes to make and break circuit, by which a temporary magnet at the receiving station is magnetized and demagnetized ; by the action of this magnet the recording pointer is caused to start on its motion athwart the receiving sheet, and moving uniformly it completes its thwart stroke at the same instant as the transmitting style.

Caselli's pantelegraph admirably effects the transmission of facsimiles. The transmitting style is carried by the motion of a heavy pendulum in an arc of constant range over a cylindrical surface on which the paper containing the

message, writing, or picture, is spread. As the swing of the pendulum begins, a similar pendulum at the receiving station begins its swing; the same break of circuit which (by demagnetizing a temporary magnet) releases one, releases the other also. The latter swings in an arc of precisely the same range, and carries a precisely similar style over a similar cylindrical surface on which is placed the prepared receiving paper. In fact, the same pendulum at either station is used for transmitting and for receiving facsimiles. Nay, not only so, but each pendulum, as it swings, serves in the work both of transmitting and recording facsimiles. As it swings one way, it travels along a line over each of two messages or drawings, while the other pendulum in its synchronous swing traces a corresponding line over each of two receiving sheets; and as it swings the other way, it traces a line on each of two receiving sheets, corresponding to the lines along which the transmitting style of the other is passing along two messages or drawings. Such, at least, is the way in which the instrument works in busy times. It can, of course, send a message, or two messages, without receiving any.\*

In Caselli's pantelegraph matters are so arranged that instead of a negative facsimile, like Fig. 9, a true facsimile is obtained in all respects except that the letters and figures are made by closely set dark lines instead of being dark throughout as in the message. The transmitting paper is conducting and the ink non-conducting, as in Bakewell's original arrangement; but instead of the conducting paper completing the circuit for the distant station, it completes a short home circuit (so to speak) along which the current travels without entering on the distant circuit. When the non-conducting ink breaks the short circuit, the current

\* It is to be noticed, however, that the recording pointer must always mark its lines in the same direction, so that, unless a message is being transmitted at the same time that one is being received (in which case the oscillations both ways are utilized), the instrument works only during one half of each complete double oscillation.

travels in the long circuit through the recording pointer at the receiving station ; and a mark is thus made corresponding to the inked part of the transmitting sheet instead of the blank part, as in the older plan.

The following passage from Guillemin's "Application of the Physical Forces" indicates the effectiveness of Caselli's pantelegraph not only as respects the character of the message it conveys, but as to rapidity of transmission. (I alter the measures from the metric to our usual system of notation.\* ) "Nothing is simpler than the writing of the pantelegraph. The message when written is placed on the surface of the transmitting cylinder. The clerk makes the warning signals, and then sets the pendulum going. The transmission of the message is accomplished automatically, without the clerk having any work to do, and consequently without [his] being obliged to acquire any special knowledge. Since two despatches may be sent at the same time—and since shorthand may be used—the rapidity of transmission may be considerable." "The long pendulum of Caselli's telegraph," says M. Quet, "generally performs about forty oscillations a minute, and the styles trace forty broken lines, separated from each other by less than the hundredth part of an inch. In one minute the lines described by the style have ranged over a breadth of more than half an inch, and in twenty minutes of nearly  $10\frac{1}{2}$  inches. As we can give the lines a length of  $4\frac{1}{4}$  inches, it follows that in twenty minutes Caselli's apparatus furnishes the facsimile of the writing or drawing traced on a metallized plate  $4\frac{1}{4}$  inches broad by  $10\frac{1}{2}$  inches long. For clearness of reproduction, the original writing must be very legible and in large characters." "Since 1865 the line from Paris to Lyons and Marseilles has been open to the public for the transmission of messages by this truly marvellous system."

\* It seems to me a pity that in the English edition of this work the usual measures have not been substituted throughout. The book is not intended or indeed suitable for scientific readers, who alone are accustomed to the metric system. Other readers do not care to have a little sum in reduction to go through at each numerical statement.

It will easily be seen that Caselli's method is capable of many important uses besides the transmission of facsimiles of handwriting. For instance, by means of it a portrait of some person who is to be indentified—whether fraudulent absconder, or escaped prisoner or lunatic, or wife who has eloped from her husband, or husband who has deserted his wife, or missing child, and so on—can be sent in a few minutes to a distant city where the missing person is likely



FIG. IO.



FIG. II.

to be. All that is necessary is that from a photograph or other portrait an artist employed for the purpose at the transmitting station should, in bold and heavy lines, sketch the lineaments of the missing person on one of the prepared sheets, as in Fig. IO. The portrait at the receiving station will appear as in Fig. II, and if necessary an artist at this station can darken the lines or in other ways improve the picture without altering the likeness.

But now we must turn to the greatest marvel of all—the transmission of tones, tunes, and words by the electric wire.

The transmission of the rhythm of an air is of course a very simple matter. I have seen the following passage from "Lardner's Museum of Science and Art," 1859, quoted as describing an anticipation of the telephone, though in reality it only shows what every one who has heard a telegraphic indicator at work must have noticed, that the click of the instrument may be made to keep time with an air. "We were in the Hanover Street Office, when there was a pause in the business operations. Mr. M. Porter, of the office at Boston—the writer being at New York—asked what tune we would have? We replied, 'Yankee Doodle,' and to our surprise he immediately complied with our request. The instrument, a Morse one, commenced drumming the notes of the tune as perfectly and distinctly as a skilful drummer could have made them at the head of a regiment, and many will be astonished to hear that 'Yankee Doodle' can travel by lightning. . . . So perfectly and distinctly were the sounds of the tunes transmitted, that good instrumental performers could have no difficulty in keeping time with the instruments at this end of the wires. . . . That a pianist in London should execute a fantasia at Paris, Brussels, Berlin, and Vienna, at the same moment, and with the same spirit, expression, and precision as if the instruments at these distant places were under his fingers, is not only within the limits of practicability, but really presents no other difficulty than may arise from the expense of the performances. From what has just been stated, it is clear that the time of music has been already transmitted, and the production of the sounds does not offer any more difficulty than the printing of the letters of a despatch." Unfortunately, Lardner omitted to describe how this easy task was to be achieved.

Reuss first in 1861 showed how a sound can be transmitted. At the sending station, according to his method, there is a box, into which, through a pipe in the side, the note to be transmitted is sounded. The box is open at the

top, and across it, near the top, is stretched a membrane which vibrates synchronously with the aerial vibrations corresponding to the note. At the middle of the membrane, on its upper surface, is a small disc of metal, connected by a thin strip of copper with the positive pole of the battery at the transmitting station. The disc also, when the machine is about to be put in use, lightly touches a point on a metallic arm, along which (while this contact continues) the electric current passes to the wire communicating with the distant station. At that station the wire is carried in a coil round a straight rod of soft iron suspended horizontally in such a way as to be free to vibrate between two sounding-boards. After forming this coil, the wire which conveys the current passes to the earth-plate and so home. As already explained, while the current passes, the rod of iron is magnetized, but the rod loses its magnetization when the current ceases.

Now, when a note is sounded in the box at the transmitting station, the membrane vibrates, and at each vibration the metal disc is separated from the point which it lightly touches when at rest. Thus contact is broken at regular intervals, corresponding to the rate of vibration due to the note. Suppose, for instance, the note *C* is sounded ; then there are 256 complete vibrations in a second, the electric current is therefore interrupted and renewed, and the bar of soft iron magnetized and demagnetized, 256 times in a second. Now, it had been discovered by Page and Henry that when a bar of iron is rapidly magnetized and demagnetized, it is put into vibrations synchronizing with the interruptions of the current, and therefore emits a note of the same tone as that which has been sounded into the transmitting box.

Professor Heisler, in his "Lehrbuch der technischen Physik," 1866, wrote of Reuss's telephone : "The instrument is still in its infancy ; however, by the use of batteries of proper strength, it already transmits not only single musical tones, but even the most intricate melodies, sung at one end of the line, to the other, situated at a great distance, and makes them perceptible there with all desirable distinctness." Dr.

Van der Weyde, of New York, states that, after reading an account of Reuss's telephone, he had two such instruments constructed, and exhibited them at the meeting of the Polytechnic Club of the American Institute. "The original sounds were produced at the furthest extremity of the large building (the Cooper Institute), totally out of hearing of the Association; and the receiving instrument, standing on the table in the lecture-room, produced, with a peculiar and rather nasal twang, the different tunes sung into the box at the other end of the line; not powerfully, it is true, but very distinctly and correctly. In the succeeding summer I improved the form of the box, so as to produce a more powerful vibration of the membrane. I also improved the receiving instrument by introducing several iron wires into the coil, so as to produce a stronger vibration. I submitted these, with some other improvements, to the meeting of the American Association for the Advancement of Science, and on that occasion (now seven years ago) expressed the opinion that the instrument contained the germ of a new method of working the electric telegraph, and would undoubtedly lead to further improvements in this branch of science."

The telephonic successes recently achieved by Mr. Gray were in part anticipated by La Cour, of Copenhagen, whose method may be thus described: At the transmitting station a tuning-fork is set in vibration. At each vibration one of the prongs touches a fine strip of metal completing a circuit. At the receiving station the wire conveying the electric current is coiled round the prongs of another tuning-fork of the same tone, but without touching them. The intermittent current, corresponding as it does with the rate of vibration proper to the receiving fork, sets this fork in vibration; and in La Cour's instrument the vibrations of the receiving fork were used to complete the circuit of a local battery. His object was not so much the production of tones as the use of the vibrations corresponding to different tones, to act on different receiving instruments. For only a fork corresponding to the sending fork could be set in vibration by the

intermittent current resulting from the latter's vibrations. So that, if there were several transmitting forks, each could send its own message at the same time, each receiving fork responding only to the vibrations of the corresponding transmitting fork. La Cour proposed, in fact, that his instrument should be used in combination with other methods of telegraphic communication. Thus, since the transmitting fork, whenever put in vibration, sets the local battery of the receiving station at work, it can be used to work a Morse instrument, or it could work an ordinary Wheatstone and Cook instrument, or it could be used for a pantelegraph. The same wire, when different forks are used, could work simultaneously several instruments at the receiving station. One special use indicated by La Cour was the adaptation of his system to the Caselli pantelegraph, whereby, instead of one style, a comb of styles might be carried over the transmitting and recording plates. It would be necessary, in all such applications of his method (though, strangely enough, La Cour's description makes no mention of the point), that the vibrations of the transmitting fork should admit of being instantly stopped or "damped."

Mr. Gray's system is more directly telephonic, as aiming rather at the development of sound itself than at the transmission of messages by the vibrations corresponding to sound. A series of tuning-forks are used, which are set in separate vibration by fingering the notes of a key-board. The vibrations are transmitted to a receiving instrument consisting of a series of reeds, corresponding in note to the series of transmitting forks, each reed being enclosed in a sounding-box. These boxes vary in length from two feet to six inches, and are connected by two wooden bars, one of which carries an electro-magnet, round the coils of which pass the currents from the transmitting instrument. When a tuning-fork is set in vibration by the performer at the transmitting key-board, the electro-magnet is magnetized and demagnetized synchronously with the vibrations of the fork. Not only are vibrations thus imparted to the reed of

corresponding note, but these are synchronously strengthened by thuds resulting from the lengthening of the iron when magnetized.

So far as its musical capabilities are concerned, Gray's telephone can hardly be regarded as fulfilling all the hopes that have been expressed concerning telephonic music. "Dreaming enthusiasts of a prophetic turn of mind foretold," we learn, "that a time would come when future Pattis would sing on a London stage to audiences in New York, Berlin, St. Petersburg, Shanghai, San Francisco, and Constantinople all at once." But the account of the first concert given at a distance scarcely realizes these fond expectations. When "Home, Sweet Home," played at Philadelphia, came floating through the air at the Steinway Hall, New York, "the sound was like that of a distant organ, rather faint, for a hard storm was in progress, and there was consequently a great leakage of the electric current, but quite clear and musical. The lower notes were the best, the higher being sometimes almost inaudible. 'The Last Rose of Summer,' 'Com' è gentil,' and other melodies, followed, with more or less success. There was no attempt to play chords," though three or four notes can be sounded together. It must be confessed that the rosy predictions of M. Strakosch (*the impresario*) "as to the future of this instrument seem rather exalted, and we are not likely as yet to lay on our music from a central reservoir as we lay on gas and water, though the experiment was certainly a very curious one."

The importance of Mr. Gray's, as of La Cour's inventions, depends, however, far more on the way in which they increase the message-bearing capacity of telegraphy than on their power of conveying airs to a distance. At the Philadelphia Exhibition, Sir W. Thomson heard four messages sounded simultaneously by the Gray telephone. The Morse alphabet was used. I have mentioned that in that alphabet various combinations of dots and dashes are used to represent different letters; it is only necessary to substitute the short and long duration of a note for dots and dashes to have a

similar sound alphabet. Suppose, now, four tuning-forks at the transmitting station, whose notes are *Do* (G), *Mi*, *Sol*, and *Do* (C), or say *C, E, G*, and *C'*, then by each

of these forks a separate message may be transmitted, all the messages being carried simultaneously by the same line to separate sounding reeds (or forks, if preferred), and received by different clerks. With a suitable key-board, a single clerk could send the four messages simultaneously, striking chords instead of single notes, though considerable practice would be necessary to transform four verbal messages at once into the proper telephonic music, and some skill in fingering to give the proper duration to each note.

Lastly, we come to the greatest achievement of all, Professor Graham Bell's vocal telephone. In the autumn of 1875 I had the pleasure of hearing from Professor Bell in the course of a ride—all too short—from Boston to Salem, Mass., an account of his instrument as then devised, and of his hopes as to future developments. These hopes have since been in great part fulfilled, but I venture to predict that we do not yet know all, or nearly all, that the vocal telephone, in Bell's hands, is to achieve.

It ought to be mentioned at the outset that Bell claims to have demonstrated in 1873 (a year before La Cour) the possibility of transmitting several messages simultaneously by means of the Morse alphabet.

Bell's original arrangement for vocal telephony was as follows:—At one station a drumhead of goldbeaters' skin, about  $2\frac{3}{4}$  inches in diameter, was placed in front of an electro-magnet. To the middle of the drumhead, on the side towards the magnet, was glued a circular piece of clock-spring. A similar electro-magnet, drumhead, etc., were placed at the other station. When notes were sung or words spoken before one drumhead, the vibrations of the goldbeaters' skin carried the small piece of clockspring

vibratingly towards and from the electro-magnet, without producing actual contact. Now, the current which was passing along the coil round the electro-magnet changed in strength with each change of position of this small piece of metal. The more rapid the vibrations, and the greater their amplitude, the more rapid and the more intense were the changes in the power of the electric current. Thus, the electro-magnet at the other station underwent changes of power which were synchronous with, and proportionate to, those changes of power in the current which were produced by the changes of position of the vibrating piece of clock-spring. Accordingly, the piece of clockspring at the receiving station, and with it the drumhead there, was caused by the electro-magnet to vibrate with the same rapidity and energy as the piece at the transmitting station. Therefore, as the drumhead at one station varied its vibrations in response to the sounds uttered in its neighbourhood, so the drumhead at the other station, varying its vibrations, emitted similar sounds. Later, the receiving drumhead was made unlike the transmitting one. Instead of a membrane carrying a small piece of metal, a thin and very flexible disc of sheet-iron, held in position by a screw, was used. This disc, set in vibration by the varying action of an electro-magnet, as in the older arrangement, uttered articulate sounds corresponding to those which, setting in motion the membrane at the transmitting station, caused the changes in the power of the electric current and in the action of the electro-magnet.

At the meeting of the British Association in 1876 Sir W. Thomson gave the following account of the performance of this instrument at the Philadelphia Exhibition :—“In the Canadian department” (for Professor Bell was not at the time an American citizen) “I heard ‘To be or not to be —there’s the rub,’ through the electric wire ; but, scorning monosyllables, the electric articulation rose to higher flights, and gave me passages taken at random from the New York newspapers :—‘S. S. Cox has arrived’ (I failed to make out

the ‘S. S. Cox’), ‘the City of New York,’ ‘Senator Morton,’ ‘the Senate has resolved to print a thousand extra copies,’ ‘the Americans in London have resolved to celebrate the coming Fourth of July.’ All this my own ears heard spoken to me with unmistakable distinctness by the thin circular disc armature of just such another little electro-magnet as this which I hold in my hand. The words were shouted with a clear and loud voice by my colleague judge, Professor Watson, at the far end of the line, holding his mouth close to a stretched membrane, carrying a piece of soft iron, which was thus made to perform in the neighbourhood of an electro-magnet, in circuit with the line, motions proportional to the sonorific motions of the air. This, the greatest by far of all the marvels of the electric telegraph, is due to a young countryman of our own, Mr. Graham Bell, of Edinburgh, and Montreal, and Boston, now about to become a naturalized citizen of the United States. Who can but admire the hardihood of invention which devised such very slight means to realize the mathematical conception that, if electricity is to convey all the delicacies of quality which distinguish articulate speech, the strength of its current must vary continuously, and as nearly as may be in simple proportion to the velocity of a particle of air engaged in constituting the sound ?”

Since these words were spoken by one of the highest authorities in matters telegraphic, Professor Bell has introduced some important modifications in his apparatus. He now employs, not an electro-magnet, but a permanent magnet. That is to say, instead of using at each station such a bar of soft iron as is shown in Fig. 6, which becomes a magnet while the electric current is passing through the coil surrounding it, he uses at each station a bar of iron permanently magnetized (or preferably a powerful magnet made of several horse-shoe bars—that is, a compound magnet), surrounded similarly by coils of wire. No battery is needed. Instead of a current through the coils magnetizing the iron, the iron already magnetized causes a current

to traverse the coils whenever it acts, or rather whenever its action changes. If an armature were placed across its ends or poles, at the moment when it drew that armature to the poles by virtue of its magnetic power, a current would traverse the coils ; but afterwards, so long as the armature remained there, there would be no current. If an armature placed near the poles were shifted rapidly in front of the poles, currents would traverse the coils, or be induced, their intensity depending on the strength of the magnet, the length of the coil, and the rapidity and range of the motions. In front of the poles of the magnet is a diaphragm of very flexible iron (or else some other flexible material bearing a small piece of iron on the surface nearest the poles). A mouthpiece to converge the sound upon this diaphragm substantially completes the apparatus at each station. Professor Bell thus describes the operation of the instrument :— “The motion of steel or iron in front of the poles of a magnet creates a current of electricity in coils surrounding the poles of the magnet, and the duration of this current of electricity coincides with the duration of the motion of the steel or iron moved or vibrated in the proximity of the magnet. When the human voice causes the diaphragm to vibrate, electrical undulations are induced in the coils around the magnets precisely similar to the undulations of the air produced by the voice. The coils are connected with the line wire, and the undulations induced in them travel through the wire, and, passing through the coils of another instrument of similar construction at the other end of the line, are again resolved into air undulations by the diaphragm of this (other) instrument.”

So perfectly are the sound undulations repeated—though the instrument has not yet assumed its final form—that not only has the lightest whisper uttered at one end of a line of 140 miles been distinctly heard at the other, but the speaker can be distinguished by his voice when he is known to the listener. So far as can be seen, there is every room to believe that before long Professor Bell’s grand invention will

be perfected to such a degree that words uttered on the American side of the Atlantic will be heard distinctly after traversing 2000 miles under the Atlantic, at the European end of the submarine cable—so that Sir W. Thomson at Valentia could tell by the voice whether Graham Bell, or Cyrus Field, or his late colleague Professor Watson, were speaking to him from Newfoundland. Yet a single wave of those which toss in millions on the Atlantic, rolling in on the Irish strand, would utterly drown the voices thus made audible after passing beneath two thousand miles of ocean.

Here surely is the greatest of telegraphic achievements. Of all the marvels of telegraphy—and they are many—none are equal to, none seem even comparable with, this one. Strange truly is the history of the progress of research which has culminated in this noble triumph, wonderful the thought that from the study of the convulsive twitchings of a dead frog by Galvani, and of the quivering of delicately poised magnetic needles by Ampère, should gradually have arisen through successive developments a system of communication so perfect and so wonderful as telegraphy has already become, and promising yet greater marvels in the future.

The last paragraph had barely been written when news arrived of another form of telephone, surpassing Gray's and La Cour's in some respects as a conveyor of musical tones, but as yet unable to speak like Bell's. It is the invention of Mr. Edison, an American electrician. He calls it the motograph. He discovered about six years ago the curious property on which the construction of the instrument depends. If a piece of paper moistened with certain chemical solutions is laid upon a metallic plate connected with the positive pole of a galvanic battery, and a platinum wire connected with the negative pole is dragged over the moistened paper, the wire slides over the paper like smooth iron over ice—the usual friction disappearing so long as the current is passing from the wire to the plate through

the paper. At the receiving station of Mr. Edison's motograph there is a resonating box, from one face of which extends a spring bearing a platinum point, which is pressed by the spring upon a tape of chemically prepared paper. This tape is steadily unwound, drawing by its friction the platinum point, and with it the face of the resonator, outwards. This slight strain on the face of the resonator continues so long as no current passes from the platinum point to the metallic drum over which the moistened tape is rolling. But so soon as a current passes, the friction immediately ceases, and the face of the resonator resumes its normal position. If then at the transmitting station there is a membrane or a very fine diaphragm (as in Reuss's or Bell's arrangement) which is set vibrating by a note of any given tone, the current, as in those arrangements, is transmitted and stopped at intervals corresponding to the tone, and the face of the resonating box is freed and pulled at the same intervals. Hence, it speaks the corresponding tone. The instrument appears to have the advantage over Gray's in range. In telegraphic communication Gray's telephone is limited to about one octave. Edison's extends from the deepest bass notes to the highest notes of the human voice, which, when magnets are employed, are almost inaudible. But Edison's motograph has yet to learn to speak.

Other telegraphic marvels might well find a place here. I might speak of the wonders of submarine telegraphy, and of the marvellous delicacy of the arrangements by which messages by the Atlantic Cable are read, and not only read, but made to record themselves. I might dwell, again, on the ingenious printing telegraph of Mr. Hughes, which sets up its own types, inks them, and prints them, or on the still more elaborate plan of the Chevalier Bonelli "for converting the telegraph stations into so many type-setting workshops." But space would altogether fail me to deal properly with these and kindred marvels. There is, however, one application of telegraphy, especially interest-

ing to the astronomer, about which I must say a few words : I mean, the employment of electricity as a regulator of time. Here again it is the principle of the system, rather than details of construction, which I propose to describe. Suppose we have a clock not only of excellent construction, but under astronomical surveillance, so that when it is a second or so in error it is set right again by the stars. Let the pendulum of this clock beat seconds ; and at each beat let a galvanic current be made and broken. This may be done in many ways—thus the pendulum may at each swing tilt up a very light metallic hammer, which forms part of the circuit when down ; or the end of the pendulum may be covered with some non-conducting substance which comes at each swing between two metallic springs in very light contact, separating them and so breaking circuit ; or in many other ways the circuit may be broken. When the circuit is made, let the current travel along a wire which passes through a number of stations near or remote, traversing at each the coils of a temporary magnet. Then, at each swing of the pendulum of the regulating clock, each magnet is magnetized and demagnetized. Thus each, once in a second, draws to itself, and then releases its armature, which is thereupon pulled back by a spring. Let the armature, when drawn to the magnet, move a lever by which one tooth of a wheel is carried forward. Then the wheel is turned at the rate of one tooth per second. This wheel communicates motion to others in the usual way. In fact, we have at each station a clock driven, *not* by a weight or spring and with a pendulum which allows one tooth of an escapement wheel to pass at each swing, but by the distant regulating clock which turns a driving wheel at the rate of one tooth per second, that is, one tooth for each swing of the regulating clock's pendulum. Each clock, then, keeps perfect time with the regulating clock. In astronomy, where it is often of the utmost importance to secure perfect synchronism of observation, or the power of noting the exact difference of time between observations

made at distant stations, not only can the same clock thus keep time for two observers hundreds of miles apart, but each observer can record by the same arrangement the moment of the occurrence of some phenomenon. For if a tape be unwound automatically, as in the Morse instrument, it is easy so to arrange matters that every second's beat of the pendulum records itself by a dot or short line on the tape, and that the observer can with a touch make (or break) contact at the instant of observation, and so a mark be made properly placed between two seconds' marks —thus giving the precise time when the observation was made. Such applications, however, though exceedingly interesting to astronomers, are not among those in which the general public take chief interest. There was one occasion, however, when astronomical time-relations were connected in the most interesting manner with one of the greatest of all the marvels of telegraphy: I mean, when the *Great Eastern* in mid-ocean was supplied regularly with Greenwich time, and this so perfectly (and therefore with such perfect indication of her place in the Atlantic), that when it was calculated from the time-signals that the buoy left in open ocean to mark the place of the cut cable had been reached, and the captain was coming on deck with several officers to look for it, the buoy announced its presence by thumping the side of the great ship.

## *THE PHONOGRAPH, OR VOICE-RECORDER.*

IN the preceding essay I have described the wonderful instrument called the telephone, which has recently become as widely known in this country as in America, the country of its first development. I propose now briefly to describe another instrument—the phonograph—which, though not a telegraphic instrument, is related in some degree to the telephone. In passing, I may remark that some, who as telegraphic specialists might be expected to know better, have described the phonograph as a telegraphic invention. A writer in the *Telegraphic Journal*, for instance, who had mistaken for mine a paper on the phonograph in one of our daily newspapers, denounced me (as the supposed author of that paper) for speaking of the possibility of crystallizing sound by means of this instrument; and then went on to speak of the mistake I (that is, said author) had made in leaving my own proper subject of study to speak of telegraphic instruments and to expatiate on the powers of electricity. In reality the phonograph has no relation to telegraphy whatever, and its powers do not in the slightest degree depend on electricity. If the case had been otherwise, it may be questioned whether the student of astronomy, or of any other department of science, should be considered incompetent of necessity to describe a telegraphic instrument, or to discuss the principles of telegraphic or electrical science. What should unquestionably be left to the specialist, is the

description of the practical effect of details of instrumental construction, and the like—for only he who is in the habit of using special instruments or classes of instrument can be expected to be competent adequately to discuss such matters.

Although, however, the phonograph is not an instrument depending, like the telephone, on the action of electricity (in some form or other), yet it is related closely enough to the telephone to make the mistake of the *Telegraphic* journalist a natural one. At least, the mistake would be natural enough for any one but a telegraphic specialist; the more so that Mr. Edison is a telegraphist, and that he has effected several important and interesting inventions in telegraphic and electrical science. For instance, in the previous article, pp. 270, 271, I had occasion to describe at some length the principles of his "Motograph." I spoke of it as "another form of telephone, surpassing Gray's and La Cour's in some respects as a conveyer of musical tones, but as yet unable to speak like Bell's . . . . in telegraphic communication." I proceeded: "Gray's telephone is limited to about one octave. Edison's extends from the deepest bass notes to the highest notes of the human voice, which, when magnets are employed, are almost inaudible; but it has yet to learn to speak."

The phonograph is an instrument which *has* learned to speak, though it does not speak at a distance like the telephone or the motograph. Yet there seems no special reason why it should not combine both qualities—the power of repeating messages at considerable intervals of time after they were originally spoken, and the power of transmitting them to great distances.

I have said that the phonograph is an instrument closely related to the telephone. If we consider this feature of the instrument attentively, we shall be led to the clearer recognition of the acoustical principles on which its properties depend, and also of the nature of some of the interesting acoustical problems on which light seems likely to be thrown by means of experiments with this instrument.

In the telephone a stretched membrane, or a diaphragm of very flexible iron, vibrates when words are uttered in its neighbourhood. When a stretched membrane is used, with a small piece of iron at the centre, this small piece of iron, as swayed by the vibrations of the membrane, causes electrical undulations to be induced in the coils round the poles of a magnet placed in front of the membrane. These undulations travel along the wire and pass through the coils of another instrument of similar construction at the other end of the wire, where, accordingly, a stretched membrane vibrates precisely as the first had done. The vibrations of this membrane excite atmospheric vibrations identical in character with those which fell upon the first membrane when the words were uttered in its neighbourhood ; and therefore the same words appear to be uttered in the neighbourhood of the second membrane, however far it may be from the transmitting membrane, so only that the electrical undulations are effectually transmitted from the sending to the receiving instrument.

I have here described what happened in the case of that earlier form of the telephone in which a stretched membrane of some such substance as goldbeater's skin was employed, at the centre of which only was placed a small piece of iron. For in its bearing on the subject of the phonograph, this particular form of telephonic diaphragm is more suggestive than the later form in which very flexible iron was employed. We see that the vibrations of a small piece of iron at the centre of a membrane are competent to reproduce all the peculiarities of the atmospheric waves which fall upon the membrane when words are uttered in its neighbourhood. This must be regarded, I conceive, as a remarkable acoustical discovery. Most students of acoustics would have surmised that to reproduce the motions merely of the central parts of a stretched diaphragm would be altogether insufficient for the reproduction of the complicated series of sound-waves corresponding to the utterance of words. I apprehend that if the problem had originally been suggested

simply as an acoustical one, the idea entertained would have been this—that though the motions of a diaphragm receiving vocal sound-waves *might* be generated artificially in such sort as to produce the same vocal sounds, yet this could only be done by first determining what particular points of the diaphragm were centres of motion, so to speak, and then adopting some mechanical arrangements for giving to small portions of the membrane at these points the necessary oscillating motions. It would not, I think, have been supposed that motions communicated to the centre of the diaphragm would suffice to make the whole diaphragm vibrate properly in all its different parts.

Let us briefly consider what was before known about the vibrations of plates, discs, and diaphragms, when particular tones were sounded in their neighbourhood ; and also what was known respecting the requirements for vocal sounds and speech as distinguished from simple tones. I need hardly say that I propose only to consider these points in a general, not in a special, manner.

We must first carefully draw a distinction between the vibrations of a plate or disc which is itself the source of sound, and those vibrations which are excited in a plate or disc by sound-waves otherwise originated. If a disc or plate of given size be set in vibration by a blow or other impulse it will give forth a special sound, according to the place where it is struck, or it will give forth combinations of the several tones which it is capable of emitting. On the other hand, experiment shows that a diaphragm like that used in the telephone—not only the electric telephone, but such common telephones as have been sold of late in large quantities in toy shops, etc.—will respond to any sounds which are properly directed towards it, not merely reproducing sounds of different tones, but all the peculiarities which characterize vocal sounds. In the former case, the size of a disc and the conditions under which it is struck determine the nature of its vibrations, and the air responds to the vibrations thus excited ; in the latter, the air is set

moving in vibrations of a special kind by the sounds or words uttered, and the disc or diaphragm responds to these vibrations. Nevertheless, though it is important that this distinction be recognized, we can still learn, from the behaviour of discs and plates set in vibration by a blow or other impulse, the laws according to which the actual motions of the various parts of a vibrating disc or plate take place. We owe to Chladni the invention of a method for rendering visible the nature of such motions.

Certain electrical experiments of Lichtenberg suggested to Chladni the idea of scattering fine sand over the plate or disc whose motions he wished to examine. If a horizontal plate covered with fine sand is set in vibration, those parts which move upwards and downwards scatter the sand from their neighbourhood, while on those points which undergo no change of position the sand will remain. Such points are called *nodes*; and rows of such points are called *nodal lines*, which may be either straight or curved, according to circumstances.

If a square plate of glass is held by a suitable clamp at its centre, and the middle point of a side is touched while a bow is drawn across the edge near a corner, the sand is seen to gather in the form of a cross dividing the square into four equal squares—like a cross of St. George. If the finger touches a corner, and the bow is drawn across the middle of a side, the sand forms a cross dividing the square along its diagonals—like a cross of St. Andrew. Touching two points equidistant from two corners, and drawing the bow along the middle of the opposite edge, we get the diagonal cross and also certain curved lines of sand systematically placed in each of the four quarters into which the diagonals divide the square. We also have, in this case, a far shriller note from the vibrating plate. And so, by various changes in the position of the points clamped by the finger and of the part of the edge along which the bow is drawn, we can obtain innumerable varieties of nodal lines and curves along which the sand gathers upon the surface of the vibrating plate.

When we take a circular plate of glass, clamped at the middle, and touching one part of its edge with the finger, draw the bow across a point of the edge half a quadrant from the finger, we see the sand arrange itself along two diameters intersecting at right angles. If the bow is drawn at a point one-third a quadrant from the finger-clamped point, we get a six-pointed star. If the bow is drawn at a point a fourth of a quadrant from the finger-clamped point, we get an eight-pointed star. And so we can get the sand to arrange itself into a star of any even number of points ; that is, we can get a star of four, six, eight, ten, twelve, etc., points, but not of three, five, seven, etc.

In these cases the centre of the plate or disc has been fixed. If, instead, the plate or disc be fixed by a clip at the edge, or clamped elsewhere than at the centre, we find the sand arranging itself into other forms, in which the centre may or may not appear ; that is, the centre may or may not be nodal, according to circumstances.

A curious effect is produced if very fine powder be strewn along with the sand over the plate. For it is found that the dust gathers, not where the nodes or places of no vibration lie, but where the motion is greatest. Faraday assigns as the cause of this peculiarity the circumstance that "the light powder is entangled by the little whirlwinds of air produced by the vibrations of the plate ; it cannot escape from the little cyclones, though the heavier sand particles are readily driven through them ; when, therefore, the motion ceases, the light powder settles down in heaps at the places where the vibration was a maximum." In proof of this theory we have the fact that "in vacuo no such effect is produced ; all powders light and heavy move to the nodal lines." (Tyndall on "Sound.")

Now if we consider the meaning of such results as these, we shall begin to recognize the perplexing but also instructive character of the evidence derived from the telephone, and applied to the construction of the phonograph. It appears that when a disc is vibrating under such special conditions

as to give forth a particular series of tones (the so-called fundamental tone of the disc and other tones combined with it which belong to its series of overtones), the various parts of the disc are vibrating to and fro in a direction square to the face of the disc, except certain points at which there is no vibration, these points together forming curves of special forms along the substance of the disc.

When, on the other hand, tones of various kinds are sounded in the neighbourhood of a disc or of a stretched circular membrane, we may assume that the different parts of the disc are set in vibration after a manner at least equally complicated. If the tones belong to the series which could be emitted by the diaphragm when struck, we can understand that the vibrations of the diaphragm would resemble those which would result from a blow struck under special conditions. When other tones are sounded, it may be assumed that the sound-waves which reach the diaphragm cause it to vibrate as though not the circumference (only) but a circle in the substance of the diaphragm—concentric, of course, with the circumference, and corresponding in dimensions with the tone of the sounds—were fixed. If a drum of given size is struck, we hear a note of particular tone. If we heard, as the result of a blow on the same drum, a much higher tone, we should know that in some way or other the effective dimensions of the drum-skin had been reduced—as for instance, by a ring firmly pressed against the inside of the skin. So when a diaphragm is responding to tones other than those corresponding to its size, tension, etc., we infer that the sound-waves reaching it cause it to behave, so far as its effective vibrating portion is concerned, as though its conformation had altered. When several tones are responded to by such a diaphragm, we may infer that the vibrations of the diaphragm are remarkably complicated.

Now the varieties of vibratory motion to which the diaphragm of the telephone has been made to respond have been multitudinous. Not only have all orders of sound singly and together been responded to, but vocal sounds which in

many respects differ widely from ordinary tones are repeated, and the peculiarities of intonation which distinguish one voice from another have been faithfully reproduced.

Let us consider in what respects vocal sounds, and especially the sounds employed in speech, differ from mere combinations of ordinary tones.

It has been said, and with some justice, that the organ of voice is of the nature of a reed instrument. A reed instrument, as most persons know, is one in which musical sounds are produced by the action of a vibrating reed in breaking up a current of air into a series of short puffs. The harmonium, accordion, concertina, etc., are reed instruments, the reed for each note being a fine strip of metal vibrating in a slit. The vocal organ of man is at the top of the windpipe, along which a continuous current of air can be forced by the lungs. Certain elastic bands are attached to the head of the windpipe, almost closing the aperture. These vocal chords are thrown into vibration by the current of air from the lungs ; and as the rate of their vibration is made to vary by varying their tension, the sound changes in tone. So far, we have what corresponds to a reed instrument admitting of being altered in pitch so as to emit different notes. The mouth, however, affects the character of the sound uttered from the throat. The character of a *tone* emitted by the throat cannot be altered by any change in the configuration of the mouth ; so that if a single tone were in reality produced by the vocal chords, the resonance of the mouth would only strengthen that tone more or less according to the figure given to the cavity of the mouth at the will of the singer or speaker. But in reality, besides the fundamental tone uttered by the vocal chords, a series of overtones are produced. Overtones are tones corresponding to vibration at twice, three times, four times, etc., the rate of the vibration producing the fundamental tone. Now the cavity of the mouth can be so modified in shape as to strengthen either the fundamental tone or any one of these overtones. And according as special tones are strengthened in this way

various vocal sounds are produced, without changing the pitch or intensity of the sound actually uttered. Calling the fundamental tone the first tone, the overtones just mentioned the second, third, fourth, etc., tones respectively (after Tyn-dall), we find that the following relations exist between the combinations of these tones and the various vowel sounds:—

If the lips are pushed forward so as to make the cavity of the mouth deep and the orifice of the mouth small, we get the deepest resonance of which the mouth is capable; the fundamental tone is reinforced, while the higher tones are as far as possible thrown into the shade. The resulting vowel sound is that of deep U ("oo" in "hoop").

If the mouth is so far opened that the fundamental tone is accompanied by a strong second tone (the next higher octave to the fundamental tone), we get the vowel sound O (as in "hole"). The third and fourth tones feebly accompanying the first and second make the sound more perfect, but are not necessary.

If the orifice of the mouth is so widened, and the volume of the cavity so reduced, that the fundamental tone is lost, the second somewhat weakened, and the third given as the chief tone, with very weak fourth and fifth tones, we have the vowel sound A.

To produce the vowel sound E, the resonant cavity of the mouth must be considerably reduced. The fourth tone is the characteristic of this vowel. Yet the second tone also must be given with moderate strength. The first and third tones must be weak, and the fifth tone should be added with moderate strength.

To produce the vowel sound A, as in "far," the higher overtones are chiefly used, the second is wanting altogether, the third feeble, the higher tones—especially the fifth and seventh—strong.

The vowel sound I, as in "fine," it should be added, is not a simple sound, but diphthongal. The two sounds whose succession gives the sound we represent (erroneously) by a single letter I (long), are not very different from "a" as in

"far," and "ee" (or "i" as in "ravine"); they, lie, however, in reality, respectively between "a" in "far" and "fat," and "i" in "ravine" and "pin." Thus the tones and overtones necessary for sounding "I" long, do not require a separate description, any more than those necessary for sounding other diphthongs, as "oi," "oe," and so forth.

We see, then, that the sound-waves necessary to reproduce accurately the various vowel sounds, are more complicated than those which would correspond to the fundamental tones simply in which any sound may be uttered. There must not only be in each case certain overtones, but each overtone must be sounded with its due degree of strength.

But this is not all, even as regards the vowel sounds, the most readily reproducible peculiarities of ordinary speech. Spoken sounds differ from musical sounds properly so called, in varying in pitch throughout their continuance. So far as tone is concerned, apart from vowel quality, the speech note may be imitated by sliding a finger up the finger-board of a violin while the bow is being drawn. A familiar illustration of the varying pitch of a speech note is found in the utterance of Hamlet's question, "Pale, or red?" with intense anxiety of inquiry, if one may so speak. "The speech note on the word 'pale' will consist of an upward movement of the voice, while that on 'red' will be a downward movement, and in both words the voice will traverse an interval of pitch so wide as to be conspicuous to ordinary ears; while the cultivated perception of the musician will detect the voice moving through a less interval of pitch while he is uttering the word 'or' of the same sentence. And he who can record in musical notation the sounds which he hears, will perceive the musical interval traversed in these vocal movements, and the place also of these speech notes on the musical staff." Variations of this kind, only not so great in amount, occur in ordinary speech; and no telephonic or phonographic instrument could be regarded as perfect, or even satisfactory, which did not reproduce them.

But the vowel sounds are, after all, combinations and modifications of musical tones. It is otherwise with consonantal sounds, which, in reality, result from various ways in which vowel sounds are commenced, interrupted (wholly or partially), and resumed. In one respect this statement requires, perhaps, some modification—a point which has not been much noticed by writers on vocal sounds. In the case of liquids, vowel sounds are not partially interrupted only, as is commonly stated. They cease entirely as vowel sounds, though the utterance of a vocal sound is continued when a liquid consonant is uttered. Let the reader utter any word in which a liquid occurs, and he will find that while the liquid itself is sounded the vowel sounds preceding or following the liquid cease entirely. Repeating slowly, for example, the word "remain," dwelling on all the liquids, we find that while the "r" is being sounded the "ē" sound cannot be given, and this sound ceases so soon as the "m" is sounded; similarly the long "a" sound can only be uttered when the "m" sound ceases, and cannot be carried on into the sound of the final liquid "n." The liquids are, in fact, improperly called semi-vowels, since no vowel sound can accompany their utterance. The tone, however, with which they are sounded can be modified during their utterance. In sounding labials the emission of air is not stopped completely at any moment. The same is true of the sibilants s, z, sh, zh, and of the consonants g, j, f, v, th (hard and soft). These are called, on this account, *continuous* consonants. The only consonants in pronouncing which the emission of air is for a moment entirely stopped, are the true mutes, sometimes called the six *explosive* consonants, b, p, t, d, k, and g.

To reproduce artificially sounds resembling those of the consonants in speech, we must for a moment interrupt, wholly for explosive and partially for continuous consonant sounds, the passage of air through a reed pipe. Tyndall thus describes an experiment of this kind in which an imperfect imitation of the sound of the letter "m" was

obtained—an imitation only requiring, to render it perfect, as I have myself experimentally verified, attention to the consideration respecting liquids pointed out in the preceding paragraph. "Here," says Tyndall, describing the experiment as conducted during a lecture, "is a free reed fixed in a frame, but without any pipe associated with it, mounted on the acoustic bellows. When air is urged through the orifice, it speaks in this forcible manner. I now fix upon the frame of the reed a pyramidal pipe; you notice a change in the clang, and, by pushing my flat hand over the open end of the pipe, the similarity between the sounds produced and those of the human voice is unmistakable. Holding the palm of my hand over the end of the pipe, so as to close it altogether, and then raising my hand twice in quick succession, the word 'mamma' is heard as plainly as if it were uttered by an infant. For this pyramidal tube I now substitute a shorter one, and with it make the same experiment. The 'mamma' now heard is exactly such as would be uttered by a child with a stopped nose. Thus, by associating with a vibrating reed a suitable pipe, we can impart to the sound of the reed the qualities of the human voice." The "m" obtained in these experiments was, however, imperfect. To produce an "m" sound such as an adult would utter without a "stopped nose," all that is necessary is to make a small opening (experiment readily determines the proper size and position) in the side of the pyramidal pipe, so that, as in the natural utterance of this liquid, the emission of air is not altogether interrupted.

I witnessed in 1874 some curious illustrations of the artificial production of vocal sounds, at the Stevens Institute, Hoboken, N.J., where the ingenious Professor Mayer (who will have, I trust, a good deal to say about the scientific significance of telephonic and phonographic experiments before long) has acoustic apparatus, including several talking-pipes. By suitably moving his hand on the top of some of these pipes, he could make them speak certain words with tolerable distinctness, and even utter short sentences.

I remember the performance closed with the remarkably distinct utterance, by one profane pipe, of the words euphemistically rendered by Mark Twain (in his story of the Seven Sleepers, I think), " Go thou to Hades ! "

Now, the speaking diaphragm in the telephone, as in the phonograph, presently to be described, must reproduce not only all the varieties of sound-wave corresponding to vowel sounds, with their intermixtures of the fundamental tone and its overtones and their inflexions or sliding changes of pitch, but also all the effects produced on the receiving diaphragm by those interruptions, complete or partial, of aerial emission which correspond to the pronunciation of the various consonant sounds. It might certainly have seemed hopeless, from all that had been before known or surmised respecting the effects of aerial vibrations on flexible diaphragms, to attempt to make a diaphragm speak artificially—in other words, to make the movements of all parts of it correspond with those of a diaphragm set in vibration by spoken words—by movements affecting only its central part. It is in the recognition of the possibility of this, or rather in the discovery of the fact that the movements of a minute portion of the middle of a diaphragm regulate the vibratory and other movements of the entire diaphragm, that the great scientific interest of Professor Graham Bell's researches appears to me to reside.

It may be well, in illustration of the difficulties with which formerly the subject appeared to be surrounded, to describe the results of experiments which preceded, though they can scarcely be said to have led up to, the invention of artificial ways of reproducing speech. I do not now refer to experiments like those of Kratzenstein of St. Petersburg, and Von Kempelen of Vienna, in 1779, and the more successful experiments by Willis in later years, but to attempts which have been made to obtain material records of the aerial motions accompanying the utterance of spoken words. The most successful of these attempts was that made by Mr. W. H. Barlow. His purpose was

“to construct an instrument which should record the pneumatic actions” accompanying the utterance of articulated sounds “by diagrams, in a manner analogous to that in which the indicator-diagram of a steam-engine records the action of the engine.” He perceived that the actual aerial pressures involved being very small and very variable, and the succession of impulses and changes of pressure being very rapid, it was necessary that the moving parts should be very light, and that the movement and marking should be accomplished with as little friction as possible. The instrument he constructed consisted of a small speaking-trumpet about four inches long, having an ordinary mouth-piece connected to a tube half an inch in diameter, the thin end of which widened out so as to form an aperture of  $2\frac{1}{4}$  inches diameter. This aperture was covered with a membrane of goldbeater’s skin, or thin gutta-percha. A spring carrying a marker was made to press against the membrane with a slight initial pressure, to prevent as far as possible the effects of jarring and consequent vibratory action. A light arm of aluminium was connected with the spring, and held the marker; and a continuous strip of paper was made to pass under the marker in the manner employed in telegraphy. The marker consisted of a small, fine sable brush, placed in a light tube of glass one-tenth of an inch in diameter, the tube being rounded at the lower end, and pierced with a hole about one-twentieth of an inch in diameter. Through this hole the tip of the brush projected, and was fed by colour put into the glass tube by which it was held. It should be added that, to provide for the escape of air passing through the speaking-trumpet, a small opening was made in the side, so that the pressure exerted upon the membrane was that due to the excess of air forced into the trumpet over that expelled through the orifice. The strength of the spring which carried the marker was so adjusted to the size of the orifice that, while the lightest pressures arising under articulation could be recorded, the greatest pressures should not produce a movement exceeding the width of the paper.

"It will be seen," says Mr. Barlow, "that in this construction of the instrument the sudden application of pressure is as suddenly recorded, subject only to the modifications occasioned by the inertia, momentum, and friction of the parts moved. But the record of the sudden cessation of pressure is further affected by the time required to discharge the air through the escape-orifice. Inasmuch, however, as these several effects are similar under similar circumstances, the same diagram should always be obtained from the same pneumatic action when the instrument is in proper adjustment; and this result is fairly borne out by the experiments."

The defect of the instrument consisted in the fact that it recorded changes of pressure only; and in point of fact it seems to result, from the experiments made with it, that it could only indicate the order in which explosive, continuant, and liquid consonants succeeded each other in spoken words, the vowels being all expressed in the same way, and only one letter—the rough R, or R with a burr—being always unmistakably indicated. The explosives were represented by a sudden sharp rise and fall in the recorded curve; the height of the rise depending on the strength with which the explosive is uttered, not on the nature of the consonant itself. Thus the word "tick" is represented by a higher elevation for the "t" than for the "k," but the word "kite" by a higher elevation for the "k" than for the "t." It is noteworthy that there is always a second smaller rise and fall after the first chief one, in the case of each of the explosives. This shows that the membrane, having first been forcibly distended by the small aerial explosion accompanying the utterance of such a consonant, sways back beyond the position where the pressure and the elasticity of the membrane would (for the moment) exactly balance, and then oscillates back again over that position before returning to its undistended condition. Sometimes a third small elevation can be recognized, and when an explosive is followed by a rolling "r" several

small elevations are seen. The continuous consonants produce elevations less steep and less high; aspirates and sibilants give rounded hills. But the results vary greatly according to the position of a consonant; and, so far as I can make out from a careful study of the very interesting diagrams accompanying Mr. Barlow's paper, it would be quite impossible to define precisely the characteristic records even of each order of consonantal sounds, far less of each separate sound.

We could readily understand that the movement of the central part of the diaphragm in the telephone should give much more characteristic differences for the various sounds than Barlow's logograph. For if we imagine a small pointer attached to the centre of the face of the receiving diaphragm while words are uttered in its neighbourhood, the end of that pointer would not only move to and fro in a direction square to the face of the diaphragm, as was the case with Barlow's marker, but it would also sway round its mean position in various small circles or ovals, varying in size, shape, and position, according to the various sounds uttered. We might expect, then, that if in any way a record of the actual motions of the extremity of that small pointer could be obtained, in such sort that its displacement in directions square to the face of the diaphragm, as well as its swayings around its mean position, would be indicated in some pictorial manner, the study of such records would indicate the exact words spoken near the diaphragm, and even, perhaps, the precise tones in which they were uttered. For Barlow's logograph, dealing with one only of the orders of motion (really triple in character), gives diagrams in which the general character of the sounds uttered is clearly indicated, and the supposed records would show much more.

But although this might, from *a priori* considerations, have been reasonably looked for, it by no means follows that the actual results of Bell's telephonic experiments could have been anticipated. That the movement of the central

part of the diaphragm should suffice to show that such and such words had been uttered, is one thing ; but that these movements should of themselves suffice, if artificially reproduced, to cause the diaphragm to reproduce these words, is another and a very different one. I venture to express my conviction that at the beginning of his researches Professor Bell can have had very little hope that any such result would be obtained, notwithstanding some remarkable experiments respecting the transmission of sound which we can *now* very clearly perceive to point in that direction.

When, however, he had invented the telephone, this point was in effect demonstrated ; for in that instrument, as we have seen, the movements of the minute piece of metal attached (at least in the earlier forms of the instrument) to the centre of the receiving membrane, suffice, when precisely copied by the similar central piece of metal in the transmitting membrane, to cause the words which produced the motions of the receiving or hearing membrane to be uttered (or seem to be uttered) by the transmitting or speaking membrane.

It was reserved, however, for Edison (of New Jersey, U.S.A., Electrical Adviser to the Western Union Telegraph Company) to show how advantage might be taken of this discovery to make a diaphragm speak, not directly through the action of the movements of a diaphragm affected by spoken words or other sounds, and therefore either simultaneously with these or in such quick succession after them as corresponds with the transmission of their effects along some line of electrical or other communication, but by the mechanical reproduction of similar movements at any subsequent time (within certain limits at present, but probably hereafter with practically unlimited extension as to time).

The following is slightly modified from Edison's own description of the phonograph :—

The instrument is composed of three parts mainly ; namely, a receiving, a recording, and a transmitting appa-

ratus. The receiving apparatus consists of a curved tube, one end of which is fitted with a mouthpiece. The other end is about two inches in diameter, and is closed with a disc or diaphragm of exceedingly thin metal, capable of being thrust slightly outwards or vibrated upon gentle pressure being applied to it from within the tube. To the centre of this diaphragm (which is vertical) is fixed a small blunt steel pin, which shares the vibratory motion of the diaphragm. This arrangement is set on a table, and can be adjusted suitably with respect to the second part of the instrument—the recorder. This is a brass cylinder, about four inches in length and four in diameter, cut with a continuous V-groove from one end to the other, so that in effect it represents a large screw. There are forty of these grooves in the entire length of the cylinder. The cylinder turns steadily, when the instrument is in operation, upon a vertical axis, its face being presented to the steel point of the receiving apparatus. The shaft on which it turns is provided with a screw-thread and works in a screwed bearing, so that as the shaft is turned (by a handle) it not only turns the cylinder, but steadily carries it upwards. The rate of this vertical motion is such that the cylinder behaves precisely as if its groove worked in a screw-bearing. Thus, if the pointer be set opposite the middle of the uppermost part of the continuous groove at the beginning of this turning motion, it will traverse the groove continuously to its lowest part, which it will reach after forty turnings of the handle. (More correctly, perhaps, we might say that the groove continuously traverses past the pointer.) Now, suppose that a piece of some such substance as tinfoil is wrapped round the cylinder. Then the pointer, when at rest, just touches the tinfoil. But when the diaphragm is vibrating under the action of aerial waves resulting from various sounds, the pointer vibrates in such a way as to indent the tinfoil—not only to a greater or less depth according to the play of the pointer to and fro in a direction square to the face of the diaphragm, but also over a range all round its mean position,

corresponding to the play of the end of the pointer around its mean position. The groove allows the pressure of the pointer against the tinfoil free action. If the cylinder had no groove the dead resistance of the tinfoil, thus backed up by an unyielding surface, would stop the play of the pointer. Under the actual conditions, the tinfoil is only kept taut enough to receive the impressions, while yielding sufficiently to let the play of the pointer continue unrestrained. If now a person speaks into the receiving tube, and the handle of the cylinder be turned, the vibrations of the pointer are impressed upon the portion of the tinfoil lying over the hollow groove, and are retained by it. They will be more or less deeply marked according to the quality of the sounds emitted, and according also, of course, to the strength with which the speaker utters the sounds, and to the nature of the modulations and inflexions of his voice. The result is a message verbally imprinted upon a strip of metal. It differs from the result in the case of Barlow's logograph, in being virtually a record in three dimensions instead of one only. The varying depth of the impressions corresponds to the varying height of the curve in Barlow's diagrams ; but there the resemblance ceases ; for that was the single feature which Barlow's logographs could present. Edison's imprinted words show, besides varying depth of impression, a varying range on either side of the mean track of the pointer, and also—though the eye is not able to detect this effect—there is a varying rate of progression according as the end of the pointer has been swayed towards or from the direction in which, owing to the motion of the cylinder, the pointer is virtually travelling.

We may say of the record thus obtained that it is sound presented in a visible form. A journalist who has written on the phonograph has spoken of this record as corresponding to the crystallization of sound. And another who, like the former, has been (erroneously, but that is a detail) identified with myself, has said, in like fanciful vein, that the story of Baron Münchhausen hearing words which had been

frozen during severe cold melting into speech again, so that all the babble of a past day came floating about his ears, has been realized by Edison's invention. Although such expressions may not be, and in point of fact are not, strictly scientific, I am not disposed, for my own part, to cavil with them. If they could by any possibility be taken *au pied de la lettre* (and, by the way, we find quite a new meaning for this expression in the light of what is now known about vowels and consonants), there would be valid objection to their use. But, as no one supposes that Edison's phonograph really crystallizes words or freezes sounds, it seems hypercritical to denounce such expressions as the critic of the *Telegraphic Journal* has denounced them.

To return to Edison's instrument.

Having obtained a material record of sounds, vocal or otherwise, it remains that a contrivance should be adopted for making this record reproduce the sounds by which it was itself formed. This is effected by a third portion of the apparatus, the transmitter. This is a conical drum, or rather a drum shaped like a frustum of a cone, having its larger end open, the smaller—which is about two inches in diameter—being covered with paper stretched tight like the parchment of a drum-head. In front of this diaphragm is a light flat steel spring, held vertically, and ending in a blunt steel point, which projects from it and corresponds precisely with that on the diaphragm of the receiver. The spring is connected with the paper diaphragm by a silken thread, just sufficiently in tension to cause the outer face of the diaphragm to be slightly convex. Having removed the receiving apparatus from the cylinder and set the cylinder back to its original position, the transmitting apparatus is brought up to the cylinder until the steel point just rests, without pressure, in the first indentation made in the tinfoil by the point of the receiver. If now the handle is turned at the same speed as when the message was being recorded, the steel point will follow the line of impression, and will vibrate in periods corresponding to the impressions which

were produced by the point of the receiving apparatus. The paper diaphragm being thus set into vibrations of the requisite kind in number, depth, and side-range, there are produced precisely the same sounds that set the diaphragm of the receiver into vibration originally. Thus the words of the speaker are heard issuing from the conical drum in his own voice, tinged with a slightly metallic or mechanical tone. If the cylinder be more slowly turned when transmitting than it had been when receiving the message, the voice assumes a base tone; if more quickly, the message is given with a more treble voice. "In the present machine," says the account, "when a long message is to be recorded, so soon as one strip of tinfoil is filled, it is removed and replaced by others, until the communication has been completed. In using the machine for the purpose of correspondence, the metal strips are removed from the cylinder and sent to the person with whom the speaker desires to correspond, who must possess a machine similar to that used by the sender. The person receiving the strips places them in turn on the cylinder of his apparatus, applies the transmitter, and puts the cylinder in motion, when he hears his friend's voice speaking to him from the indented metal. And he can repeat the contents of the missive as often as he pleases, until he has worn the metal through. The sender can make an infinite number of copies of his communication by taking a plaster-of-Paris cast of the original, and rubbing off impressions from it on a clean sheet of foil."

I forbear from dwelling further on the interest and value of this noble invention, or of considering some of the developments which it will probably receive before long, for already I have occupied more space than I had intended. I have no doubt that in these days it will bring its inventor less credit, and far less material gain, than would be acquired from the invention of some ingenious contrivance for destroying many lives at a blow, bursting a hole as large as a church door in the bottom of an ironclad, or in some other way helping men to carry out those destructive instincts which

they inherit from savage and brutal ancestors. But hereafter, when the representatives of the brutality and savagery of our nature are held in proper disesteem, and those who have added new enjoyments to life are justly valued, a high place in the esteem of men will be accorded to him who has answered one half of the poet's aspiration,

"Oh for the touch of a vanished hand,  
And the sound of a voice that is still!"

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NOTE.—Since the present paper was written, M. Aurel de Ratti has made some experiments which he regards as tending to show that there is no mechanical vibration. Thus, "when the cavities above and below the iron disc of an ordinary telephone are filled with wadding, the instrument will transmit and speak with undiminished clearness. On placing a finger on the iron disc opposite the magnet, the instrument will transmit and speak distinctly, only ceasing to act when sufficient pressure is applied to bring plate and magnet into contact. Connecting the centre of the disc by means of a short thread with an extremely sensitive membrane, no sound is given out by the latter when a message is transmitted. Bringing the iron cores of the double telephone in contact with the disc, and pressing with the fingers against the plate on the other side, a weak current from a Daniell cell produced a distinct click in the plate, and on drawing a wire from the cell over a file which formed part of the circuit, a rattling noise was produced in the instrument." If these experiments had been made before the phonograph was invented, they would have suggested the impracticability of constructing any instrument which would do what the phonograph actually does, viz., cause sounds to be repeated by exciting a merely mechanical vibration of the central part of a thin metallic disc. But as the phonograph proves that this can actually be done, we must conclude that M. Aurel de Ratti's experiments will not bear the interpretation he places upon them. They show, nevertheless, that exceedingly minute vibrations of probably a very small portion of the telephonic disc suffice for the distinct transmission of vocal sounds. This might indeed be inferred from the experiments of M. Demozet, of Nantes, who finds that the vibrations of the transmitting telephone are in amplitude little more than 1-2000th those of the receiving telephone.

## *THE GORILLA AND OTHER APES.*

ABOUT twenty-five centuries ago, a voyager called Hanno is said to have sailed from Carthage, between the Pillars of Hercules—that is, through the Straits of Gibraltar—along the shores of Africa. “Passing the Streams of Fire,” says the narrator, “we came to a bay called the Horn of the South. In the recess there was an island, like the first, having a lake, and in this there was another island full of wild men. But much the greater part of them were women, with hairy bodies, whom the interpreters called ‘Gorillas.’ Pursuing them, we were not able to take the men; they all escaped, being able to climb the precipices; and defended themselves with pieces of rock. But three women, who bit and scratched those who led them, were not willing to follow. However, having killed them, we flayed them, and conveyed the skins to Carthage; for we did not sail any further, as provisions began to fail.”\*

In the opinion of many naturalists, the wild men of this story were the anthropoid or manlike apes which are now called gorillas, rediscovered recently by Du Chaillu. The region inhabited by the gorillas is a well-wooded country, “extending about a thousand miles from the Gulf of Guinea southward,” says Gosse; “and as the gorilla is not found beyond these limits, so we may pretty conclusively infer that the extreme point of Hanno was some-

\* Hanno’s *Periplus*—the voyage of Hanno, chief of the Carthaginians, round the parts of Libya, beyond the Pillars of Hercules, the narrative of which he posted up in the Temple of Kronos.

where in this region." I must confess these inferences seem to me somewhat open to question, and the account of Hanno's voyage only interesting in its relation to the gorilla, as having suggested the name now given to this race of apes. It is not probable that Hanno sailed much further than Sierra Leone; according to Rennell, the island where the "wild men" were seen, was the small island lying close to Sherbro, some seventy miles south of Sierra Leone. To have reached the gorilla district after doubling Cape Verd—which is itself a point considerably south of the most southerly city founded by Hanno—he would have had to voyage a distance exceeding that of Cape Verd from Carthage. Nothing in the account suggests that the portion of the voyage, after the colonizing was completed, had so great a range. The behaviour of the "wild men," again, does not correspond with the known habits of the gorilla. The idea suggested is that of a species of anthropoid ape far inferior to the gorilla in strength, courage, and ferocity.

The next accounts which have been regarded as relating to the gorilla are those given by Portuguese voyagers. These narratives have been received with considerable doubt, because in some parts they seem manifestly fabulous. Thus the pictures representing apes show also huge flying dragons with a crocodile's head; and we have no reason for believing that batlike creatures like the pterodactyls of the greensand existed in Africa or elsewhere so late as the time of the Portuguese voyages of discovery. Purchas, in his history of Andrew Battell, speaks of "a kinde of great apes, if they might so bee termed, of the height of a man, but twice as bigge in feature of their limmes, with strength proportionable, hairie all over, otherwise altogether like men and women in their whole bodily shape, except that their legges had no calves." This description accords well with the peculiarities of gorillas, and may be regarded as the first genuine account of these animals. Battell's contemporaries called the apes so described Pongoes. It is probable that in selecting the name Pongo for the young

gorilla lately at the Westminster Aquarium, the proprietors of this interesting creature showed a more accurate judgment of the meaning of Purchas's narrative than Du Chaillu showed of Hanno's account, in calling the great anthropoid ape of the Gulf of Guinea a gorilla.

I propose here briefly to sketch the peculiarities of the four apes which approach nearest in form to man—the gorilla, the chimpanzee, the orang-outang, and the gibbon; and then, though not dealing generally with the question of our relationship to these non-speaking (and, in some respects, perhaps, "unspeakable") animals, to touch on some points connected with this question, and to point out some errors which are very commonly entertained on the subject.

It may be well, in the first place, to point out that the terms "ape," "baboon," and "monkey" are no longer used as they were by the older naturalists. Formerly the term "ape" was limited to tailless simians having no cheek-pouches, and the same number of teeth as man; the term "baboon" to short-tailed simians with dog-shaped heads; and the term "monkey," unless used generically, to the long-tailed species. This was the usage suggested by Ray, and adopted systematically thirty or forty years ago. But it is no longer followed, though no uniform classification has been substituted for the old arrangement. Thus Mivart divides the apes into two classes—calling the first the *Simiadæ*, or Old World apes; and the second the *Cebidæ*, or New World apes. He subdivides the *Simiadæ* into (1) the *Siminæ*, including the gorilla, chimpanzee, orang, and gibbon; (2) the *Semnopithecinae*; and (3) the *Cynopithecinae*; neither of which subdivisions will occupy much of our attention here, save as respects the third subdivision of the *Cynopithecinae*, viz., the *Cynocephali*, which includes the baboons. The other great division, the *Cebidæ*, or New World apes, are for the most part very unlike the Old World apes. None of them approach the gorilla or orang-outang in size; most of them are long-tailed; and the

number and arrangement of the teeth is different. The feature, however, which most naturalists have selected as the characteristic distinction between the apes of the Old World and of the New World is the position of the nostrils. The former are called Catarhine, a word signifying that the nostrils are directed downwards; the latter are called Platyrrhine, or broad-nosed. The nostrils of all the Old World apes are separated by a narrow cartilaginous plate or septum, whereas the septum of the New World apes is broad. After the apes come, according to Mivart's classification, the half-apes or lemuroids.

I ought, perhaps, to have mentioned that Mivart, in describing the lemuroids as the second sub-order of a great order of animals, the Primates, speaks of a man as (zoologically speaking) belonging to the first sub-order. So that, in point of fact, the two sub-orders are the Anthropoids, including the *Anthropos* (man) and the Lemuroids, including the lemur.

The classification here indicated will serve our present purpose very well. But the reader is reminded that, as already mentioned, naturalists do not adopt at present any uniform system of classification. Moreover, the term *Simiadæ* is usually employed—and will be employed here—to represent the entire simian race, *i.e.*, both the Simiadæ and the Cebidæ of Mivart's classification.

And now, turning to the Anthropoid apes, we find ourselves at the outset confronted by the question, Which of the four apes, the gorilla, the orang-outang, the chimpanzee, or the gibbon, is to be regarded as nearest to man in intelligence? So far as bodily configuration is concerned, our opinion would vary according to the particular feature which we selected for consideration. But it will probably be admitted that intelligence should be the characteristic by which our opinions should be guided.

The gibbon may be dismissed at once, though, as will presently appear, there are some features in which this ape resembles man more closely than either the gorilla, the orang-outang, or the chimpanzee.

The gorilla must, I fear, be summarily ejected from the position of honour to which he has been raised by many naturalists. Though the gorilla is a much larger animal than the chimpanzee, his brain barely equals the chimpanzee's in mass. It is also less fully developed in front. In fact, Gratiolet asserts that of all the broad-chested apes, the gorilla is—so far as brain character is concerned—the lowest and most degraded. He regards the gorilla's brain as only a more advanced form of that of the brutal baboons, while the orang's brain is the culminating form of the gibbon type, and the chimpanzee's the culminating form of the macaque type. This does not dispose of the difficulty very satisfactorily, however, because it remains to be shown whether the gibbon type and the macaque type are superior as types to the baboon types. But it may suffice to remark that the baboons are all brutal and ferocious, whereas the gibbons are comparatively gentle animals, and the macaques docile and even playful. It may be questioned whether brutality and ferocity should be regarded as necessarily removing the gorilla further from man; because it is certain that the races of man which approach nearest to the anthropoid apes, with which races the comparison should assuredly be made, are characterized by these very qualities, brutality and ferocity. Intelligence must be otherwise gauged. Probably the average proportion of the brain's weight to that of the entire body, and the complexity of the structure of the brain, would afford the best means of deciding the question. But, unfortunately, we have very unsatisfactory evidence on these points. The naturalists who have based opinions on such evidence as has been obtained, seem to overlook the poverty of the evidence. Knowing as we do how greatly the human brain varies in size and complexity, not only in different races, but in different individuals of the same race, it appears unsatisfactory in the extreme to regard the average of the brains of each simian species hitherto examined as presenting the true average cerebral capacity for each species.

Still it seems tolerably clear that the choice as to the race of apes which must be regarded as first in intelligence, and therefore as on the whole the most manlike, rests between the orang-outang and the chimpanzee. "In the world of science, as in that of politics," said Professor Rolleston in 1862, "France and England have occasionally differed as to their choice between rival candidates for royalty. If either hereditary claims or personal merits affect at all the right of succession, beyond a question the gorilla is but a pretender, and one or other of the two (other) candidates the true prince. There is a graceful as well as an ungraceful way of withdrawing from a false position, and the British public will adopt the graceful course by accepting forthwith and henceforth the French candidate"—the orang-outang. If this were intended as prophecy, it has not been fulfilled by the event, for the gorilla is still regarded by most British naturalists as the ape which comes on the whole nearest to man; but probably, in saying "the British public will adopt the graceful course" in accepting the orang-outang as "the king of the Simiadæ," Professor Rolleston meant only that that course would be graceful if adopted.

Before the discovery of the gorilla, the chimpanzee was usually regarded as next to man in the scale of the animal creation. It was Cuvier who first maintained the claim of the orang-outang to this position. Cuvier's opinion was based on the greater development of the orang-outang's brain, and the height of its forehead. But these marks of superiority belong to the orang only when young. The adult orang seems to be inferior, or at least not superior, to the chimpanzee as respects cerebral formation, and in other respects seems less to resemble man. The proportions of his body, his long arms, high shoulders, deformed neck, and imperfect ears are opposed to its claims to be regarded as manlike. In all these respects, save one, the chimpanzee seems to be greatly its superior. (The ear of the chimpanzee is large, and not placed as with us: that of the gorilla is much more like man's.)

As to the intelligence exhibited in the conduct of the chimpanzee and orang-outang, various opinions may be formed according to the various circumstances under which the animals are observed. The following has been quoted in evidence of the superiority of the chimpanzee in this respect:—About fifty years ago, a young chimpanzee and an orang-outang of about the same age were exhibited together at the Egyptian Hall. The chimpanzee, though in a declining state of health, and rendered peevish and irritable by bodily suffering, exhibited much superior marks of intelligence to his companion; he was active, quick, and observant of everything that passed around him; no new visitor entered the apartment in which he was kept, and no one left it, without attracting his attention. The orang outang, on the contrary, exhibited a melancholy and a disregard of passing occurrences almost amounting to apathy; and though in the enjoyment of better health, was evidently much inferior to his companion in quickness and observation. On one occasion, when the animals were dining on potatoes and boiled chicken, and surrounded as usual with a large party of visitors, the orang-outang allowed her plate to be taken without exhibiting the least apparent concern. Not so, however, the chimpanzee. We took advantage of an opportunity when his head was turned (to observe a person coming in) to secrete his plate also. For a few seconds he looked round to see what had become of it, but, not finding it, began to pout and fret exactly like a spoiled child, and perceiving a young lady, who happened to be standing near him, laughing, perhaps suspecting her to be the delinquent, he flew at her in the greatest rage, and would probably have bitten her had she not got beyond his reach. Upon having his plate restored, he took care to prevent the repetition of the joke by holding it firmly with one hand, while he fed himself with the other.”

This story can hardly be regarded as deciding the question in favour of the chimpanzee. Many animals, admittedly far inferior to the lowest order of monkeys in intelligence,

show watchfulness over their food, and as much ill-temper when deprived of it, and as much anxiety to recover it, as this chimpanzee did. A hundred cases in point might readily be cited.\*

Perhaps the soundest opinion respecting the relative position of the gorilla, chimpanzee, and orang-outang with reference to man, is that which places the gorilla nearest to the lower tribes of man now inhabiting Africa, the chimpanzee approximating, but not so closely, to higher African tribes, and the orang-outang approximating, but still less closely, to Asiatic tribes. It appears to me that, whatever weight naturalists may attach to those details in which the gorilla and the chimpanzee are more removed from man than the orang, no one who takes a *general* view of these three races of anthropoid apes can hesitate to regard the gorilla as that which, on the whole, approaches nearest to man; but it is to a much lower race of man that the gorilla approximates, so that the chimpanzee and the orang-outang may fairly be regarded as higher in the scale of animal life.

If we consider young specimens of the three animals, which is, on the whole, the safest way of forming an opinion, we are unmistakably led, in my judgment, to such a conclusion. I have seen three or four young chimpanzees, two young orangs, and the young gorilla lately exhibited at the Aquarium (where he could be directly compared with the chimpanzee), and I cannot hesitate to pronounce Pongo

\* I may mention one which occurred within my own experience. A mastiff of mine, some years ago, was eating from a plate full of broken meat. It was his custom to bury the large pieces when there was more than he could get through. While he was burying a large piece, a cat ran off with a small fragment. The moment he returned to the plate he missed this, and, seeing no one else near the plate, he, in his own way, accused a little daughter of mine (some two or three years old) of the theft. Looking fiercely at her, he growled his suspicions, and would not suffer her to escape from the corner where his plate stood until I dragged him away by his chain. Nor did he for some time forget the wrong which he supposed she had done him, but always growled when she came near his house.

altogether the most human of the three. A young chimpanzee reminds one rather of an old man than of a child, and the same may be said of young orangs ; but the young gorilla unmistakably reminds one of the young negro. Repeatedly, while watching Pongo, I was reminded of the looks and behaviour of young negroes whom I had seen in America, though not able in every case to fix definitely on the feature of resemblance which recalled the negro to my mind. (The reader is, doubtless, familiar with half-remembered traits such as those I refer to—traits, for instance, such as assure you that a person belongs to some county or district, though you may be unable to say what feature, expression, or gesture suggests the idea.) One circumstance may be specially noted, not only as frequently recurring, but as illustrating the traits on which the resemblance of the gorilla (when young, at any rate) to the negro depends. A negro turns his eyes where a Caucasian would turn his head. The peculiarity is probably a relic of savage life ; for the savage, whether engaged in war or in the chase, avoids, as far as possible, every movement of body or limb. Pongo looked in this way. When he thus cast his black eyes sideways at an object I found myself reminded irresistibly of the ways of the watchful negro waiters at an American hotel. Certainly there is little in the movements of the chimpanzee to remind one of any kind of human child. He is impish ; but not the most impish child of any race or tribe ever had ways, I should suppose, resembling his.

The four anthropoid apes, full grown and in their native wilds, differ greatly from each other in character. It may be well to consider their various traits, endeavouring to ascertain how far diversities existing among them may be traced to the conditions under which the four orders subsist.

The gorilla occupies a well-wooded country extending along the coast of Africa from the Gulf of Guinea southwards across the equator. When full grown he is equal to a man in height, but much more powerfully built. "Of specimens shot by Du Chaillu," says Rymer Jones, "the largest male

seems to have been at least six feet two in height ; so that, making allowance for the shortness of the lower limbs, the dimensions of a full-grown male may be said to equal those of a man of eight or nine feet high, and it is only in their length that the lower limbs are disproportionate to the gigantic trunk. In the thickness and solidity of their bones, and in the strength of their muscles, these limbs are quite in keeping with the rest of the body. When upright, the gorilla's arms reach to his knees ; the hind hands are wide, and of amazing size and power ; the great toe or thumb measures six inches in circumference ; the palms and soles, and the naked part of the face, are of an intense black colour, as is also the breast. The other parts are thickly clothed with hair of an iron grey, except the head, on which it is reddish brown, and the arms, where it is long and nearly black. The female is wholly tinged with red."

Du Chaillu gives the following account of the aspect of the gorilla in his native woods :—" Suddenly, as we were yet creeping along in a silence which made even a heavy breath seem loud and distinct, the woods were at once filled with a tremendous barking roar ; then the underbrush swayed rapidly just ahead, and presently stood before us an immense gorilla. He had gone through the jungle on all-fours ; but when he saw our party he erected himself and looked us boldly in the face. He stood about a dozen yards from us, and was a sight I think I shall never forget. Nearly six feet high (he proved four inches shorter), with immense body, huge chest, and great muscular arms, with fiercely glaring, large, deep-grey eyes, and a hellish expression of face, which seemed to me some night-mare vision ; thus stood before us the king of the African forest. He was not afraid of us ; he stood there and beat his breasts with his large fists till it resounded like an immense bass drum (which is their mode of bidding defiance), meantime giving vent to roar after roar."

The gorilla is a fruit-eater, but as fierce as the most carnivorous animals. He is said to show an enraged enmity against men, probably because he has found them not only

hostile to himself, but successful in securing the fruits which the gorilla loves, for he shows a similar hatred to the elephant, which also seeks these fruits. We are told that when the gorilla "sees the elephant busy with his trunk among the twigs, he instantly regards this as an infraction of the laws of property, and, dropping silently down to the bough, he suddenly brings his club smartly down on the sensitive finger of the elephant's proboscis, and drives off the alarmed animal trumpeting shrilly with rage and pain." His enmity to man is more terribly manifested. "The young athletic negroes in their ivory-haunts," says Gosse, "well know the prowess of the gorilla. He does not, like the lion, sullenly retreat on seeing them, but swings himself rapidly down to the lower branches, courting the conflict, and clutches the nearest of his enemies. The hideous aspect of his visage (his green eyes flashing with rage) is heightened by the thick and prominent brows being drawn spasmodically up and down, with the hair erect, causing a horrible and fiendish scowl. Weapons are torn from their possessor's grasp, gun-barrels bent and crushed in by the powerful hands and vice-like teeth of the enraged brute. More horrid still, however, is the sudden and unexpected fate which is often inflicted by him. Two negroes will be walking through one of the woodland paths unsuspecting of evil, when in an instant one misses his companion, or turns to see him drawn up in the air with a convulsed choking cry, and in a few minutes dropped to the ground, a strangled corpse. The terrified survivor gazes up, and meets the grin and glare of the fiendish giant, who, watching his opportunity, had suddenly put down his immense hind hand, caught the wretch by the neck with resistless power, and dropped him only when he ceased to struggle."

The chimpanzee inhabits the region from Sierra Leone to the southern confines of Angola, possibly as far as Cape Negro, so that his domain includes within it that of the gorilla. He attains almost the same height as the gorilla when full grown, but is far less powerfully built. In fact, in

general proportions the chimpanzee approaches man more nearly than does any other animal. His body is covered with long black coarse hair, thickest on the head, shoulders, and back, and rather thin on the breast and belly. The face is dark brown and naked, as are the ears, except that long black whiskers adorn the animal's cheeks. The hair on the forearms is directed towards the elbows, as is the case with all the anthropoid apes, and with man himself. This hair forms, where it meets the hair from the upper arm, a small ruff about the elbow joint. The chimpanzees live in society in the woods, constructing huts from the branches and foliage of trees to protect themselves against the sun and heavy rains. It is said by some travellers that the chimpanzee walks upright in its native woods, but this is doubtful ; though certainly the formation of the toes better fits them to stand upright than either the gorilla or the orang. They arm themselves with clubs, and unite to defend themselves against the attacks of wild beasts, "compelling," it is said, "even the elephant himself to abandon the districts in which they reside." We learn that "it is dangerous for men to enter their forests, unless in companies and well armed ; women in particular are often said to be carried away by these animals, and one negress is reported to have lived among them for the space of three years, during which time they treated her with uniform kindness, but always prevented any attempt on her part to escape. When the negroes leave a fire in the woods, it is said that the chimpanzees will gather round and warm themselves at the blaze, but they have not sufficient intelligence to keep it alive by fresh supplies of fuel."

The orang-outang inhabits Borneo, Java, Sumatra, and other islands of the Indian coast. He attains a greater height than the gorilla, but, though very powerful and active, would probably not be a match for the gorilla in a single combat. His arms are by comparison as well as actually much longer. Whereas the gorilla's reach only to the knees, the arms of the orang-outang almost reach the ground when

the animal is standing upright. The orang does not often assume an upright attitude, however. "It seldom attempts to walk on the hind feet alone, and when it does the hands are invariably employed for the purpose of steadyng its tottering equilibrium, touching the ground lightly on each side as it proceeds, and by this means recovering the lost balance of the body." The gorilla uses his hands differently. He can scarcely be said to walk on all-fours, because he does not place the inside of the hand on the ground, but walks on the knuckles, evidently trying to keep the fore part of the body as high as possible. "The muzzle is somewhat long, the mouth ill-shaped, the lips thin and protuberant ; the ears are very small, the chin scarcely recognizable, and the nose only to be recognized by the nostrils. The face, ears, and inside of the hands of the orang are naked and of a brick-red colour ; the fore parts are also but thinly covered with hair ; but the head, shoulders, back, and extremities are thickly clothed with long hair of dark wine-red colour, directed forwards on the crown of the head and upwards towards the elbows on the forearms."

The orang-outang changes remarkably in character and appearance as he approaches full growth. "Though exhibiting in early youth a rotundity of the cranium and a height of forehead altogether peculiar, and accompanied at the same time with a gentleness of disposition and a gravity of manners which contrast strongly with the petulant and irascible temper of the lower orders of quadrumanous mammals, the orang-outang in its adult state is even remarkable for the flatness of its retiring forehead, the great development of the superorbital and occipital crests, the prominence of its jaws, the remarkable size of its canine teeth, and the whole form of the skull, which from the globular shape of the human head, as in the young specimen, assumes all the forms and characters belonging to that of a large carnivorous animal. The extraordinary contrasts thus presented in the form of the skull at different epochs of the same animal's life were long considered as the characters of distinct species ;

nor was it till intermediate forms were obtained, exhibiting in some degree the peculiarities of both extremes, that they were finally recognized as distinguishing different periods of growth only."

Unlike the gorilla, which attacks man with peculiar malignity, and the chimpanzee, which when in large troops assails those who approach its retreats, the orang, even in its adult state, seems not to be dangerous unless attacked. Even then he does not always show great ferocity. The two following anecdotes illustrate well its character. The first is from the pen of Dr. Abel Clarke (fifth volume of the "Asiatic Researches"); the other is from Wallace's interesting work, "The Malay Archipelago." An orang-outang fully seven feet high was discovered by the company of a merchant ship, at a place called Ramboon, on the north-west coast of Sumatra, on a spot where there were few trees and little cultivated ground. "It was evident that he had come from a distance, for his legs were covered with mud up to the knees, and the natives were unacquainted with him. On the approach of the boat's crew he came down from the tree in which he was discovered, and made for a clump at some distance; exhibiting, as he moved, the appearance of a tall man-like figure, covered with shining brown hair, walking erect, with a waddling gait, but sometimes accelerating his motion with his hands, and occasionally impelling himself forward with the bough of a tree. His motion on the ground was evidently not his natural mode of progression, for, even when assisted by his hands and the bough, it was slow and vacillating; it was necessary to see him among the trees to estimate his strength and agility. On being driven to a small clump, he gained by one spring a very lofty branch and bounded from one branch to another with the swiftness of a common monkey, his progress being as rapid as that of a swift horse. After receiving five balls his exertions relaxed, and, reclining exhausted against a branch, he vomited a quantity of blood. The ammunition of the hunters being by this time exhausted, they were obliged to

fell the tree in order to obtain him ; but what was their surprise to see him, as the tree was falling, effect his retreat to another, with seemingly undiminished vigour ! In fact, they were obliged to cut down all the trees before they could force him to combat his enemies on the ground, and when finally overpowered by numbers, and nearly in a dying state, he seized a spear made of supple wood, which would have withstood the strength of the stoutest man, and broke it like a reed. It was stated, by those who aided in his death, that the human-like expression of his countenance and his piteous manner of placing his hands over his wounds, distressed their feelings so as almost to make them question the nature of the act they were committing. He was seven feet high, with a broad expanded chest and narrow waist. His chin was fringed with a beard that curled neatly on each side, and formed an ornamental rather than a frightful appendage to his visage. His arms were long even in proportion to his height, but his legs were much shorter. Upon the whole, he was a wonderful beast to behold, and there was more about him to excite amazement than fear. His hair was smooth and glossy, and his whole appearance showed him to be in the full vigour of his youth and strength." On the whole, the narrative seems to suggest a remark similar to one applied by Washington Irving to the followers of Ojeda and their treatment of the (so-called) Indians of South America, "we confess we feel a momentary doubt whether the arbitrary appellation of 'brute' is always applied to the right party."

The other story also presents man as at least as brutal as the orang concerned in the event. "A few miles down the river," says Wallace, "there is a Dyak house, and the inhabitants saw a large orang feeding on the young shoots of a palm by the river-side. On being alarmed he retreated towards the jungle which was close by, and a number of the men, armed with spears and choppers, ran out to intercept him. The man who was in front tried to run his spear through the animal's body ; but the orang seized it in his hands, and in an instant got hold of the man's arm, which

he seized in his mouth, making his teeth meet in the flesh above the elbow, which he tore and lacerated in a dreadful manner. Had not the others been close behind, the man would have been more seriously injured, if not killed, as he was quite powerless ; but they soon destroyed the creature with their spears and choppers. The man remained ill for a long time, and never fully recovered the use of his arm."

The term gibbon includes several varieties of tail-less, long-armed, catarhine apes. The largest variety, called the siamang, need alone be described here.

The siamang inhabits Sumatra. It presents several points of resemblance to the orang-outang, but is also in several respects strongly distinguished from that animal. The arms are longer even than the orang's, and the peculiar use which the orang makes of his long arms is more strikingly shown in the progression of the long-armed siamang, for the body inclining slightly forward, when the animal is on level ground the long arms are used somewhat like crutches, and they advance by jerks resembling the hobbling of a lame man whom fear compels to make an extraordinary effort. The skull is small, and much more depressed than that of the orang or chimpanzee. The face is naked and black, straggling red hairs marking the eyebrows. The eyes are deeply sunk, a peculiarity which, by the way, seems characteristic of arboreal creatures generally ;\* the nose broad and flat,

\* It may be suggested, in passing, that the association which has been commonly noticed between prominent eyeballs and command of language (phrenologists place the organ of language, in their unscientific phraseology, behind the eyeballs) may be related in some degree to the circumstance that in gradually emerging from the condition of an arboreal creature the anthropoid ape would not only cease to derive advantage from sunken eyes, but would be benefited by the possession of more prominent eyeballs. The increasing prominence of the eyeballs would thus be a change directly associated with the gradual advance of the animal to a condition in which, associating into larger and larger companies and becoming more and more dependent on mutual assistance and discipline, they would require the use of a gradually extending series of vocal signs to indicate their wants and wishes to each other.

with wide-open nostrils ; the cheeks sunk under high cheekbones ; the chin almost rudimentary. “The hair over the whole body is thick, long, and of a glossy black colour, much closer on the shoulders, back, and limbs than on the belly, which, particularly in the females, is nearly naked. The ears are entirely concealed by the hair of the head ; they are naked, and, like all the other naked parts, of a deep black colour. Beneath the chin there is a large, bare sac, of a lax and oily appearance, which is distended with air when the animal cries, and in that state resembles an enormous goître. It is similar to that possessed by the orang-outang, and undoubtedly assists in swelling the volume of the voice, and producing those astounding cries which, according to Duvancelle’s account, may be heard at the distance of several miles.” This, however, may be doubted, for M. Duvancelle himself remarks of the wouwou, that, “though deprived of the guttural sac so remarkable in the siamang, its cry is very nearly the same ; so that it would appear that this organ does not produce the effect of increasing the sound usually attributed to it, or else that it must be replaced in the wouwou by some analogous formation.”

The habits of the siamang are interesting, especially in their bearing on the relationship between the various orders of anthropoid apes and man ; for, though the gibbon is unquestionably the lowest of the four orders of the anthropoid apes in intelligence, it possesses some characteristics which bring it nearer to man (so far as they are concerned) than any of its congeners. The chief authorities respecting the ways of the siamang are the French naturalists Diard and Duvancelle, and the late Sir Stamford Raffles.

The siamangs generally assemble in large troops, “conducted, it is said, by a chief, whom the Malays believe to be invulnerable, probably because he is more agile, powerful, and difficult to capture than the rest.” “Thus united,” proceeds M. Duvancelle (in a letter addressed to Cuvier), “the siamangs salute the rising and the setting sun with the most terrific cries” (like sun-worshippers), “which

may be heard at the distance of many miles, and which, when near, stun when they do not frighten. This is the morning call of the mountain Malays; but to the inhabitants of the town, who are unaccustomed to it, it is an unsupportable annoyance. By way of compensation, the siamangs keep a profound silence during the day, unless when interrupted in their repose or their sleep. They are slow and heavy in their gait, wanting confidence when they climb and agility when they leap, so that they may be easily caught when they can be surprised. But nature, in depriving them of the means of readily escaping danger, has endowed them with a vigilance which rarely fails them; and if they hear a noise which is unusual to them, even at the distance of a mile, fright seizes them and they immediately take flight. When surprised on the ground, however, they may be captured without resistance, either overwhelmed with fear or conscious of their weakness and the impossibility of escaping. At first, indeed, they endeavour to avoid their pursuers by flight, and it is then that their want of skill in this exercise becomes most apparent."

"However numerous the troop may be, if one is wounded it is immediately abandoned by the rest, unless, indeed, it happen to be a young one. Then the mother, who either carries it or follows close behind, stops, falls with it, and, uttering the most frightful cries, precipitates herself upon the common enemy with open mouth and arms extended. But it is manifest that these animals are not made for combat; they neither know how to deal nor to shun a blow. Nor is their maternal affection displayed only in moments of danger. The care which the females bestow upon their offspring is so tender and even refined, that one would be almost tempted to attribute the sentiment to a rational rather than an instinctive process. It is a curious and interesting spectacle, which a little precaution has sometimes enabled me to witness, to see these females carry their young to the river, wash their faces in spite of their outcries, wipe and dry them, and altogether bestow

upon their cleanliness a time and attention that in many cases the children of our own species might well envy. The Malays related a fact to me, which I doubted at first, but which I consider to be in a great measure confirmed by my own subsequent observations. It is that the young siamangs, whilst yet too weak to go alone, are always carried by individuals of their own sex, by their fathers if they are males, and by their mothers if females. I have also been assured that these animals frequently become the prey of the tiger, from the same species of fascination which serpents are said to exercise over birds, squirrels, and other small animals. Servitude, however long, seems to have no effect in modifying the characteristic defects of this ape—his stupidity, sluggishness, and awkwardness. It is true that a few days suffice to make him as gentle and contented as he was before wild and distrustful; but, constitutionally timid, he never acquires the familiarity of other apes, and even his submission appears to be rather the result of extreme apathy than of any degree of confidence or affection. He is almost equally insensible to good or bad treatment; gratitude and revenge are equally strange to him."

We have next to consider certain points connected with the theory of the relationship between man and the anthropoid apes. It is hardly necessary for me to say, perhaps, that in thus dealing with a subject requiring for its independent investigation the life-long study of departments of science which are outside those in which I have taken special interest, I am not pretending to advance my opinion as of weight in matters as yet undetermined by zoologists. But it has always seemed to me, that when those who have made special study of a subject collect and publish the result of their researches, and a body of evidence is thus made available for the general body scientific, the facts can be advantageously considered by students of other branches of science, so only that, in leaving for a while their own subject, they do not depart from the true scientific method, and that they are specially careful to distinguish

what has been really ascertained from what is only surmised with a greater or less degree of probability.

In the first place, then, I would call attention to some very common mistakes respecting the Darwinian theory of the Descent of Man. I do not refer here to ordinary misconceptions respecting the theory of natural selection. To say the truth, those who have not passed beyond *this* stage of error,—those who still confound the theory of natural selection with the Lamarckian and other theories (or rather hypotheses \*) of evolution,—are not as yet in a position to deal with our present subject, and may be left out of consideration.

The errors to which I refer are in the main included in the following statement. It is supposed by many, perhaps by most, that according to Darwin man is descended from one or other of the races of anthropoid apes; and that the various orders and sub-orders of apes and monkeys at present existing can be arranged in a series gradually approaching more and more nearly to man, and indicating the various steps (or some of them, for gaps exist in the series) by which man was developed. Nothing can be plainer, however, than Darwin's contradiction of this genealogy for the human races. Not only does he not for a moment countenance the belief that the present races of monkeys and apes can be arranged in a series gradually approximating more and more nearly to man, not only does he reject the belief that man is descended from any present existing anthropoid ape, but he even denies that the progenitor of man resembled any known ape. "We must not fall into the error of supposing," he says, "that the early

\* The word hypothesis is too often used as though it were synonymous with theory, so that Newton's famous saying, "Hypotheses non fingo" has come to be regarded by many as though it expressed an objection on Newton's part against the formation of theories. This would have been strange indeed in the author of the noblest theory yet propounded by man in matters scientific. Newton indicates his meaning plainly enough, in the very paragraph in which the above expression occurs, defining an hypothesis as an opinion not based on phenomena.

progenitor of the whole simian stock, including man, was identical with, or even closely resembled, any existing ape or monkey."

It appears to me, though it may seem somewhat bold to express this opinion of the views of a naturalist so deservedly eminent as Mr. Mivart, that in his interesting little treatise, "Man and Apes,"—a treatise which may be described as opposed to Darwin's special views but not generally opposed to the theory of evolution,—he misapprehends Darwin's position in this respect. For he arrives at the conclusion that if the Darwinian theory is sound, then "low down" (*i.e.*, far remote) "in the scale of Primates" (tri-syllabic) "was an ancestral form so like man that it might well be called an *homunculus*; and we have the virtual pre-existence of man's body supposed, in order to account for the actual first appearance of that body as we know it—a supposition manifestly absurd if put forward as an explanation."\*

How, then, according to the Darwinian theory, is man related to the monkey? The answer to this question is simply that the relationship is the same in kind, though not the same in degree, as that by which the most perfect Caucasian race is related to the lowest race of Australian, or Papuan, or Bosjesman savages. No one supposes that one of these races of savages could by any process of evolution, however long-continued, be developed into a race resembling the Caucasian in bodily and mental attributes.

\* I find it somewhat difficult to understand clearly Mr. Mivart's own position with reference to the general theory of evolution. He certainly is an evolutionist, and as certainly he considers natural selection combined with the tendency to variation (as ordinarily understood) insufficient to account for the existence of the various forms of animal and vegetable existence. He supplies the missing factor in "an innate law imposed on nature, by which new and definite species, under definite conditions, emerged from a latent and potential being into actual and manifest existence;" and, so far as can be judged, he considers that the origin of man himself is an instance of the operation of this law.

Nor does any one suppose that the savage progenitor of the Caucasian races was identical with, or even closely resembled, any existing race of savages. Yet we recognize in the lowest forms of savage man our blood relations. In other words, it is generally believed that if our genealogy, and that of any existing race of savages, could be traced back through all its reticulations, we should at length reach a race whose blood we share with that race. It is also generally believed (though for my own part I think the logical consequences of the principle underlying all theories of evolution is in reality opposed to the belief) that, by tracing the genealogical reticulations still further back, we should at length arrive at a single race from which all the present races of man and no other animals have descended. The Darwinian faith with respect to men and monkeys is precisely analogous. It is believed that the genealogy of every existent race of monkeys, if traced back, would lead us to a race whose blood we share with that race of monkeys; and—which is at once a wider and a more precise proposition—that, as Darwin puts it, “the two main divisions of the Simiadæ, namely, the catarhine and platyrhine monkeys, with their sub-groups, have all proceeded from some one extremely ancient progenitor.” This proposition is manifestly wider. I call it also more precise, because it implies, and is evidently intended by Darwin to signify, that from that extremely ancient progenitor no race outside the two great orders of Simiadæ have even partially *descended*, though other races share with the Simiadæ descent from some still more remote race of progenitors.

This latter point, however, is not related specially to the common errors respecting the Darwinian theory which I have indicated above, except in so far as it is a detail of the actual Darwinian theory. I would, in passing, point out that, like the detail referred to in connection with the relationship of the various races of man, this one is not logically deducible from the theory of evolution. In fact, I have sometimes thought that the principal difficulties of that

theory arise from this unnecessary and not logically sound doctrine. I pointed out, rather more than three years ago, in an article "On some of our Blood Relations," in a weekly scientific journal, that the analogy between the descent of races and the descent of individual members of any race, requires us rather to believe that the remote progenitor of the human race and the Simiadæ has had its share—though a less share—in the generation of other races related to these in more or less remote degrees. I may perhaps most conveniently present the considerations on which I based this conclusion, by means of a somewhat familiar illustration:—

Let us take two persons, brother and sister (whom let us call the pair A), as analogues of the human race. Then these two have four great-grandparents on the father's side, and four on the mother's side. All these may be regarded as equally related to the pair A. Now, let us suppose that the descendants of the four families of great-grandparents intermarry, no marriages being in any case made outside these families, and that the descendants in the same generation as the pair A are regarded as corresponding to the entire order of the Simiadæ, the pair A representing, as already agreed, the race of man, and all families outside the descendants of the four great-grandparental families corresponding to orders of animals more distantly related than the Simiadæ to man. Then we have what corresponds (so far as our illustration is concerned) to Darwin's views respecting man and the Simiadæ, and animals lower in the scale of life. The first cousins of the pair A may be taken as representing the anthropoid apes; the second cousins as representing the lemurs or half-apes; the third cousins as representing the platyrhine or American apes. The entire family—including the pair A, representing man—is descended also, in accordance with the Darwinian view, from a single family of progenitors, no outside families sharing *descent*, though all share *blood*, with that family.

But manifestly, this is an entirely artificial and improbable arrangement in the case of families. The eight grand-

parents *might* be so removed in circumstances from surrounding families—so much superior to them, let us say—that neither they nor any of their descendants would intermarry with these inferior families: and thus none of their great-grandchildren would share descent from some other stock contemporary with the great-grandparents; or—which is the same thing, but seen in another light—none of the contemporaries of the great-grandchildren would share descent from the eight grandparents. But so complete a separation of the family from surrounding families would be altogether exceptional and unlikely. For, even assuming the eight families to be originally very markedly distinguished from all surrounding families, yet families rise and fall, marry unequally, and within the range of a few generations a wide disparity of blood and condition appears among the descendants of any group of families. So that, in point of fact, the relations assumed to subsist between man, the Simiadæ, and lower animal forms, corresponds to an unusual and improbable set of relations among families of several persons. Either, then, the relations of families must be regarded as not truly analogous to the relations of races, which no evolutionist would assert, or else we must adopt a somewhat different view of the relationship between man, the Simiadæ, and inferior animals.

One other illustration may serve not only to make my argument clearer, but also, by presenting an actual case, to enforce the conclusion to which it points.

We know that the various races of man are related together more or less closely, that some are purer than others, and that one or two claim almost absolute purity. Now, if we take one of these last, as, for instance, the Jewish race, and trace the race backwards to its origin, we find it, according to tradition, carried back to twelve families, the twelve sons of Jacob and their respective wives. (We cannot go further back because the wives of Jacob's sons must be taken into account, and they were not descended from Abraham or Isaac and their wives only,—in fact, could not

have been.) If the descendants of those twelve families had never intermarried with outside families in such sort that the descendants of such mixed families came to be regarded as true Hebrews, we should have in the Hebrews a race corresponding to the Simiadæ as regarded by Darwin, *i.e.*, a race entirely descended from one set of families, and so constituting, in fact, a single family. But we know that, despite the objections entertained by the Hebrews against the intermixture of their race with other races, this did not happen. Not only did many of those regarded as true Hebrews share descent from nations outside their own, but many of those regarded as truly belonging to nations outside the Jewish race shared descent from the twelve sons of Jacob.

The case corresponding, then, to that of the purest of all human races, and the case therefore most favourable to the view presented by Darwin (though very far from essential to the Darwinian theory), is simply this, that, in the first place, many animals regarded as truly Simiadæ share descent from animals outside that family which Darwin regards as the ape progenitor of man; and, in the second place, many animals regarded as outside the Simiadæ share descent from that ape-like progenitor. This involves the important inference that the ape-like progenitor of man was not so markedly differentiated from other families of animals then existing, that fertile intercourse was impossible. A little consideration will show that this inference accords well with, if it might not almost have been directly deduced from, the Darwinian doctrine that all orders of mammals were, in turn, descended from a still more remote progenitor race. The same considerations may manifestly be applied also to that more remote race, to the still more remote race from which all the vertebrates have descended, and so on to the source itself from which all forms of living creatures are supposed to have descended. A difficulty meets us at that remotest end of the chain analogous to the difficulty of understanding how life began at all; but we should profit little by extending

the inquiry to these difficulties, which remain, and are likely long to remain, insuperable.

So far, however, are the considerations above urged from introducing any new or insuperable objection to the Darwinian theory, that, rightly understood, they indicate the true answer to an objection which has been urged by Mivart and others against the belief that man has descended from some ape-like progenitor.

Mivart shows that no existing ape or monkey approaches man more nearly in all respects than other races, but that one resembles man more closely in some respects, another in others, a third in yet others, and so forth. "The ear lobe of the gorilla makes him our cousin," he says, "but his tongue is eloquent in his own dispraise." If the "bridging convolutions of the orang['s brain] go to sustain his claim to supremacy, they also go far to sustain a similar claim on the part of the long-tailed thumbless spider-monkeys. If the obliquely ridged teeth of *Simia* and *Troglodytes* (the chimpanzee) point to community of origin, how can we deny a similar community of origin, as thus estimated, to the howling monkeys and galagos? The liver of the gibbons proclaims them almost human; that of the gorilla declares him comparatively brutal. The lower American apes meet us with what seems the 'front of Jove himself,' compared with the gigantic but low-browed denizens of tropical Western Africa."

He concludes that the existence of these wide-spread signs of affinity and the associated signs of divergence, disprove the theory that the structural characters existing in the human frame have had their origin in the influence of inheritance and "natural selection." "In the words of the illustrious Dutch naturalists, Messrs. Schroeder, Van der Kolk and Vrolik," he says, "the lines of affinity existing between different Primates construct rather a network than a ladder. It is indeed a tangled web, the meshes of which no naturalist has as yet unravelled by the aid of natural selection. Nay, more, these complex affinities form such a

net for the use of the teleological *retiarius* as it will be difficult for his Lucretian antagonist to evade, even with the countless turns and doublings of Darwinian evolutions."

It appears to me that when we observe the analogy between the relationships of individuals, families, and races of man, and the relationships of the various species of animals, the difficulty indicated by Mr. Mivart disappears. Take, for instance, the case of the eight allied families above considered. Suppose, instead of the continual intermarriages before imagined—an exceptional order of events, be it remembered—that the more usual order of things prevails, viz., that alliances take place with other families. For simplicity, however, imagine that each married pair has two children, male and female, and that each person marries once and only once. Then it will be found that the pair A have ten families of cousins, two first-cousin families, and eight second-cousin families; these are all the families which share descent from the eight great-grandparents of the pair. (To have third-cousin families we should have to go back to the fourth generation.) Thus there are eleven families in all. Now, in the case first imagined of constant intermarrying, there would still have been eleven families, but they would all have descended from eight great-grandparents, and we should then expect to find among the eleven families various combinations, so to speak, of the special characteristics of the eight families from which they had descended. On the other hand, eleven families, in no way connected, have descended from eighty-eight great-grandparents, and would present a corresponding variety of characteristics. But in the case actually supposed, in which the eleven families are so related that each one (for what applies to the pair A applies to the others) has two first-cousin families, and eight second-cousin families, it will be found that instead of 88 they have only 56 great-grandparents, or ancestors, in the third generation above them. The two families related as first cousins to the pair A have, like these, eight great-grandparents, four out of these eight for one family, being

the four grandparents of the father of the pair A, the other four being outsiders ; while four of the eight great-grandparents of the other family of first cousins are the four grandparents of the mother of the pair A, the other four being outsiders. The other eight families each have eight great-grandparents ; two of the families having among their great-grandparents the parents of one of the grandfathers of the pair A, but no other great-grandparent in common with the pair A ; other two of the eight families having among their great-grandparents the parents of the other grandfather of the pair A ; other two having among their great-grandparents the parents of one of the grandmothers of the pair A ; the remaining two families having among their great-grandparents the parents of the other grandmother of the pair A ; while in all cases the six remaining great-grandparents of each family are outsiders, in no way related, on our assumption, either to the eight great-grandparents of the pair A or to each other, except as connected in pairs by marriage.

Now manifestly in such a case, which, save for the symmetry introduced to simplify its details, represents fairly the usual relationships between any family, its first cousins, and its second cousins, we should not expect to find any one of the ten other families resembling the pair A more closely in *all* respects than would any other of the ten. The two first-cousin families would *on the whole* resemble the pair A more nearly than would any of the other eight, but we should expect to find *some* features or circumstances in which one or other or all of the second-cousin families would show a closer resemblance to one or other or both of the pair A. This is found often, perhaps generally, to be the case, even as respects the ordinary characteristics in which resemblance is looked for, as complexion, height, features, manner, disposition, and so forth. Much more would it be recognized, if such close investigation could be made among the various families as the naturalist can make into the characteristics of men and animals. The fact, then, that features of re-

semblance to man are found, not all in one order of the Simiadæ, but scattered among the various orders, is perfectly analogous with the laws of resemblance recognized among the various members of more or less closely related families.

The same result follows if we consider the analogy between various different species of animals on the one hand and between various races of the human family on the other. No one thinks of urging against the ordinary theory that men form only a single species, the objection that none of the other families of the human race can be regarded as the progenitor of the Caucasian family, seeing that though the Mongolian type is nearer in some respects, the Ethiopian is nearer in others, the American in others, the Malay in yet others. We find in this the perfect analogue of what is recognized in the relationships between families all belonging to one nation, or even to one small branch of a nation. Is it not reasonable, then, to find in the corresponding features of scattered resemblance observed among the various branches of the great Simian family, not the objection which Mivart finds against the theory of relationship, but rather what should be expected if that theory is sound, and therefore, *pro tanto*, a confirmation of the theory?

But now, in conclusion, let us briefly consider the great difficulty of the theory that man is descended from some ape-like, arboreal, speechless animal,—the difficulty of bridging over the wide gap which confessedly separates the lowest race of savages from the highest existing race of apes. After all that has been done to diminish the difficulty, it remains a very great one. It is quite true that what is going on at this present time shows how the gap has been widened, and therefore indicates how it may once have been comparatively small. The more savage races of man are gradually disappearing on the one hand, the most man-like apes are being destroyed on the other,—so that on both sides of the great gap a widening process is at work. Ten thousand years hence the least civilized human race will probably be little inferior to the average Caucasian races of the present day,

the most civilized being far in advance of the most advanced European races of our time. On the other hand, the gorilla, the chimpanzee, the orang-outang, and the gibbon will probably be extinct or nearly so. True, the men of those days will probably have very exact records of the characteristics not only of the present savage races of man, but of the present races of apes. Nay, they will probably know of intermediate races, long since extinct even now, whose fossil remains geologists hope to discover before long as they have already discovered the remains of an ape as large as man (*the Dryopithecus*) which existed in Europe during the Miocene period;\* and more recently the remains of a race of monkeys akin to *Macacus*, which once inhabited Attica. But although our remote descendants will thus possess means which we do not possess of bridging the gap between the highest races of apes and the lowest races of man, the gap will nevertheless be wider in their time. And tracing backwards the process, which, thus traced forward, shows a widened gap, we see that once the gap must have been much narrower than it is. Lower races of man than any now known once existed on the earth, and also races of apes nearer akin to man than any now existing, even if the present races of apes are not the degraded descendants of races which, living under more favourable conditions, were better developed after their kind than the gorilla, chimpanzee, orang, and gibbon of the present time.

It may be, indeed, that in the consideration last suggested we may find some assistance in dealing with our difficult problem. It is commonly assumed that the man-like apes are the most advanced members of the Simian family save man alone, and so far as their present condition is concerned this may be true. But it is not necessarily the case

\* The Middle Tertiary period—the Tertiary, which includes the Eocene, Miocene, and Pliocene periods, being the latest of the three great periods recognized by geologists as preceding the present era, which includes the entire history of man as at present known geologically.

that the anthropoid apes have advanced to their present condition. Judging from the appearance of the young of these races, we may infer with some degree of probability that these apes are the degraded representatives of more intelligent and less savage creatures. Whereas the young of man is decidedly more savage in character than the well-nurtured and carefully trained adult, the young of apes are decidedly less savage than the adult. The same reasoning which leads us to regard the wildness, the natural cruelty, the destructiveness, the love of noise, and many other little ways of young children, as reminders of a more or less remote savage ancestry, should lead us to regard the comparative tameness and quiet of the young gorilla, for example, as evidence that in remote times the progenitors of the race were not so wild and fierce as the present race of gorillas.

But even when all such considerations, whether based on the known or the possible, have been taken into account, the gap between the lowest savage and the highest ape is not easily bridged. It is easier to see how man *may* have developed from an arboreal, unspeaking animal to his present state, than to ascertain how any part of the development was actually effected; in other words, it is easier to suggest a general hypothesis than to establish an even partial theory.

That the progenitor of man was arboreal in his habits seems altogether probable. Darwin recognizes in the arrangement of the hair on the human forearm the strongest evidence on this point, so far as the actual body of man is concerned; the remaining and perhaps stronger evidence being derived from appearances recognized in the unborn child. He, who usually seems as though he could overlook nothing, seems to me to have overlooked a peculiarity which is even more strikingly suggestive of original arboreal habits. There is one set of muscles, and, so far as I know, one only, which the infant uses freely, while the adult scarcely uses them at all. I mean the muscles which separate the toes, and those, especially, which work the big toe. Very

young children not only move the toes apart, so that the great toe and the little toe will be inclined to each other (in the plane of the sole) nearly ninety degrees, but also distinctly clutch with the toes. The habit has no relation to the child's actual means of satisfying its wants. I have often thought that the child's manner of clutching with its fingers is indicative of the former arboreal habits of the race, but it is not difficult to explain the action otherwise. The clutching movement of the toes, however, cannot be so explained. The child can neither bring food to its mouth in this way nor save itself from falling; and as the adult does not use the toes in this way the habit cannot be regarded as the first imperfect effort towards movements subsequently useful. In fact, the very circumstance that the movement is gradually disused shows that it is useless to the human child in the present condition of the race. In the very young gorilla the clutching motion of the toes is scarcely more marked than it is in a very young child; only in the gorilla the movement, being of use, is continued by the young, and is developed into that effective clutch with the feet which has been already described. Here we have another illustration of that divergency which, rather than either simple descent or ascent, characterizes the relationship between man and the anthropoid ape. In the growing gorilla a habit is more and more freely used, which is more and more completely given up by the child as he progresses towards maturity.

Probably the arboreal progenitor of man was originally compelled to abandon his arboreal habits by some slow change in the flora of his habitat, resulting in the diminution and eventual disappearance of trees suited for his movements. He would thus be compelled to adopt, at first, some such course as the chimpanzee—making huts of such branches and foliage as he could conveniently use for the purpose. The habit of living in large companies would (as in the case of the chimpanzee) become before long necessary, especially if the race or races thus driven from their former

abode in the trees were, like the gibbons, unapt when alone both in attack and in defence. One can imagine how the use of vocal signals of various kinds would be of service to the members of these troops, not only in their excursions, but during the work of erecting huts or defences against their enemies. If in two generations the silent wild dog acquires, when brought into the company of domestic dogs, no less than five distinct barking signals, we can well believe that a race much superior in intelligence, and forced by necessity to associate in large bodies, would—in many hundreds of generations, perhaps—acquire a great number of vocal symbols. These at first would express various emotions, as of affection, fear, anxiety, sympathy, and so forth. Other signals would be used to indicate the approach of enemies, or as battle-cries. I can see no reason why gradually the use of particular vocal signs to indicate various objects, animate or inanimate, and various actions, should not follow after a while. And though the possession and use of many, even of many hundreds, of such signs would be very far from even the most imperfect of the languages now employed by savage races, one can perceive the possibility—which is all that at present we can expect to recognize—that out of such systems of vocal signalling a form of language might arise, which, undergoing slow and gradual development, should, in the course of many generations, approach in character the language of the lowest savage races. That from such a beginning language should attain its higher and highest developments is not more wonderful in kind, though much more wonderful, perhaps, in degree, than that from the first imperfect methods of printing should have arisen the highest known developments of the typographic art. The real difficulty lies in conceiving how mere vocal signalling became developed into what can properly be regarded as spoken language.

Of the difficulties related to the origin of, or rather the development of, man's moral consciousness, space will not permit me to speak, even though there were much to be said

beyond the admission that these difficulties have not as yet been overcome. It must be remembered, however, that races of men still exist whose moral consciousness can hardly be regarded as very fully developed. Not only so, but, through a form of reversion to savage types, the highest and most cultivated races of man bring forth from time to time (as our police reports too plainly testify) beings utterly savage, brutal, and even ("which is else") bestial. Nay, the man is fortunate who has never had occasion to control innate tendencies to evil which are at least strongly significant of the origin of our race. To most minds it must be pleasanter as certainly it seems more reasonable, to believe that the evil tendencies of our race are manifestations of qualities undergoing gradual extinction, than to regard them as the consequences of one past offence, and so to have no reason for trusting in their gradual eradication hereafter. But, as Darwin says, in the true scientific spirit, "We are not here concerned with hopes or fears, only with the truth as far as our reason allows us to discover it. We must acknowledge that man, with all his noble qualities, with sympathy which feels for the most debased, with benevolence which extends not only to other men but to the humblest living creature, with his God-like intellect which has penetrated into the movements and constitution of the solar system,—with all these exalted powers, man still bears in his bodily frame the indelible stamp of his lowly origin." As it seems to me, man's moral nature teaches the same lesson with equal, if not greater, significance.

## THE USE AND ABUSE OF FOOD.

FRANCIS BACON has laid it down as an axiom that experiment is the foundation of all real progress in knowledge. "Man," he said, "as the minister and interpreter of nature, does and understands as much as his observations on the order of nature permit him, and neither knows nor is capable of more."\* It would seem, then, as if there could be no subject on which man should be better informed than on the value of various articles of food, and the quantity in which each should be used. On most branches of experimental inquiry, a few men in each age—perhaps but for a few ages in succession—have pursued for a longer or shorter portion of their life, a system of experiment and observation. But on the subject of food or diet all men in all ages have been practical experimenters, and not for a few years only, but during their entire life. One would expect, then, that no questions could be more decisively settled than those which relate to the use or the abuse of food. Every one ought to know, it might be supposed, what kinds of food are good for the health, in what quantity each should be taken, what changes of diet tend to correct this or that kind of ill-health, and how long each change should be continued.

Unfortunately, as we know, this is far from being the case. We all eat many things which are bad for us, and omit to eat many things which would be good for us. We

\* Closely following in this respect his illustrious namesake Roger, who writes, in the sixth chapter of his *Opus Majus*, "Sine experientia nihil sufficienter sciri potest."

change our diet, too often, without any consideration, or from false considerations, of the wants of the body. When we have derived benefit from some change of diet, we are apt to continue the new diet after the necessity for it has passed away. As to quantity, also, we seldom follow well-judged rules. Some take less nutriment (or less of some particular form of nutriment) than is needed to supply the absolute requirements of the system; others persistently overload the system, despite all the warnings which their own experience and that of others should afford of the mischief likely to follow that course.

It is only of late years that systematic efforts have been made to throw light on the subject of the proper use of food, to distinguish between its various forms, and to analyze the special office of each form. I propose to exhibit, in a popular manner, some of the more important practical conclusions to which men of science have been led by their investigations into these questions.

The human body has been compared to a lamp in which a flame is burning. In some respects the comparison is a most apt one, as we shall see presently. But man does more than *live*; he *works*,—with his brain or with his muscles. And therefore the human frame may be more justly compared to a steam-engine than to the flame of a lamp. Of mere life, the latter illustration is sufficiently apt, but it leaves unillustrated man's capacity for work; and since food is taken with two principal objects—the maintenance of life and the renewal of material used up in brain work and muscular work—we shall find that the comparison of man to a machine affords a far better illustration of our subject than the more common comparisons of the life of man to a burning flame, and of food to the fuel which serves to maintain combustion.

There is, however, one class of food, and, perhaps, on the whole, the most important, the operation of which is equally well illustrated by either comparison. The sort of food to which I refer may be termed *heat-maintaining* food.

I distinguish it thus from food which serves other ends, but of course it is not to be understood that any article of diet serves *solely* the end of maintaining heat. Accordingly, we find that heat-maintaining substance exists in nearly all the ordinary articles of food. Of these there are two—sugar and fat—which may be looked on as special “heat-givers.” Starch, also, which appears in all vegetables, and thus comes to form a large proportion of our daily food, is a heat-giver. In fact, this substance only enters the system in the form of sugar, the saliva having the power of converting starch (which is insoluble in water) into sugar, and thus rendering it soluble and digestible.

Starch, as I have said, appears in all vegetables. But it is found more freely in some than in others. It constitutes nearly the whole substance of arrowroot, sago, and tapioca, and appears more or less freely in potatoes, rice, wheat, barley, and oats. In the process of vegetation it is converted into sugar; and thus it happens that vegetable diet—whether presenting starch in its natural form to be converted into sugar by the consumer, or containing sugar which has resulted from a process of change undergone by starch—is in general heat-maintaining. Sugar is used as a convenient means of maintaining the heat-supply; for in eating sugar we are saved the trouble of converting starch into sugar. A love for sweet things is the instinctive expression of the necessity for heat-maintaining food. We see this liking strongly developed in children, whose rapid growth is continually drawing upon their heat-supply. So far as adults are concerned, the taste for sweet food is found to prevail more in temperate than in tropical climes, as might be expected; but, contrary to what we might at first expect, we do not find any increase in the liking for sweet food in very cold climates. Another and a more effective way of securing the required heat-supply prevails in such countries.

As starch is converted into sugar, so by a further process sugar is converted into fat. It is by the conversion of sugar into fat that its heat-supplying power is made available.

This conversion takes place in the vegetable as well as in the animal system, and thus fat appears in a variety of forms—as butter, suet, oil, and so forth. Now, precisely as sugar is a more convenient heat-supplier than starch, so fat exceeds sugar in its power of maintaining animal heat. It has been calculated that one pound of fat—whether in the form of suet, butter, or oil—will go as far towards the maintenance of animal heat as two pounds of sugar, or as two pounds and a half of starch. Thus it happens that in very cold countries there is developed a taste for such articles of food as contain most fat, or even for pure fat and its analogues—oil, butter, tallow, dripping, and other forms of *grease*.

I have spoken of starch, sugar, and fat as heat-forming articles of food; but I must note their influence in the development of muscles and nerves. Without a certain proportion of fat in the food a wasting of the tissues will always take place; for muscles and nerves cannot form without fat. And conversely, the best remedy for wasting diseases is to be found in the supply of some easily digestible form of fatty food. Well-fatted meat, and especially meat in which the fat is to be seen distributed through the flesh, may be taken under such circumstances. Butter and salad oil are then also proper articles of food. Cream is still better, and cream cheeses may be used with advantage. It is on account of its heat-supplying and fat-forming qualities that cod-liver oil has taken its place as one of the most valuable remedies for scrofulous and consumptive patients.

But it must be noted that the formation of fat is not the object with which heat-supplying food is taken. It is an indication of derangement of the system when heat-giving food is too readily converted into fat. And in so far as this process of conversion takes place beyond what is required for the formation of muscles and nerves, the body suffers in the loss of its just proportion of heat-supply. Of course, if too large an amount of heat-giving food is taken into the system, we may expect that the surplus will be deposited in

the form of adipose tissue. The deposition of fat in such a case will be far less injurious to the system than an excessive heat-supply would be. But when only a just amount of heat-giving food is taken, and in place of fulfilling its just office this food is converted into adipose tissue, it becomes necessary to inquire into the cause of the mischief. Technically, the evil may be described as resulting from the deficient oxygenation of the heat-supplying food. This generally arises from defective circulation, and may often be cured by a very moderate but *systematic* increase in the amount of daily exercise, or by the use of the sponge-bath, or, lastly, by such changes in the dress—and especially in the articles of attire worn next to the skin—as tend to encourage a freer circulation of the blood. The tendency to accumulate fat may sometimes be traced to the use of over-warm coverings at night, and especially to the use of woollen night-clothes. By attending to considerations of this sort, more readily and safely than by an undue diminution of the amount of heat-supplying food, the tendency to obesity may frequently be corrected.

In warm weather we should diminish the supply of heat-giving food. In such weather the system does not require the same daily addition to its animal heat, and the excess is converted into fat. Experiments have shown that despite the increased rate at which perspiration proceeds during the summer months, men uniformly fed throughout the year increase in weight in summer and lose weight in winter.

So far as mere existence is concerned, heat-forming food may be looked upon as the real fuel on which the lamp of life is sustained. But man, considerd as a working being, cannot exist without *energy-forming* food. All work, whether of the brain or of the limbs, involves the exhaustion of nervous and muscular matter; and unless the exhausted matter be renewed, the work must come to an end. The supply of heat-giving food may be compared to the supply of fuel for the fire of a steam-engine. By means of this supply the *fire* is kept alive; but if the fire have nothing to

work upon, its energies are wasted or used to the injury of the machine itself. The supply of water, and its continual use (in the form of steam) in the propulsion of the engine, are the processes corresponding to the continual exhaustion and renewal of the muscles and nerves of the human frame, And the comparison may be carried yet further. We see that in the case of the engine the amount of smoke, or rather of carbonic acid, thrown out by the blast-pipe is a measure of the vital energy (so to speak) within the engine ; but the amount of work done by the engine is measured rather by the quantity of steam which is thrown out, because the elastic force of every particle of steam has been exerted in the propulsion of the engine before being thrown out through the blast-pipe. In a manner precisely corresponding to this, the amount of carbonic acid gas exhaled by a man is a measure of the rate at which mere existence is proceeding ; but the amount of work, mental or muscular, which the man achieves, is measured by the amount of used-up brain-material and muscle-material which is daily thrown off by the body. I shall presently show in what way this amount is estimated.

In the composition of the muscles there is a material called *fibrine*, and in the composition of the nerves there is a material called *albumen*. These are the substances\* which are exhausted during mental and bodily labour, and which have to be renewed if we are to continue working with our head or with our hands. Nay more, life itself involves work ; the heart, the lungs, the liver, each internal organ of the body, performs its share of work, just as a certain proportion of the power of a steam-engine is expended in merely moving the machinery which sets it in action. If the waste of material involved in this form of work is not compensated

\* Fibrine and albumen are identical in composition. *Caseine*, which is the coagulable portion of milk, is composed in the same manner. The chief distinction between the three substances consists in their mode of coagulation ; fibrine coagulating spontaneously, albumen under the action of heat, and caseine by the action of acetic acid.

by a continual and sufficient supply of fibrine and albumen, the result will be a gradual lowering of all the powers of the system, until some one or other gives way,—the heart ceases to beat, or the stomach to digest, or the liver to secrete bile,—and so death ensues.

The fibrine and albumen in the animal frame are derived exclusively from vegetables. For although we seem to derive a portion of the supply from animal food, yet the fibrine and albumen thus supplied have been derived in the beginning from the vegetable kingdom. “It is the peculiar property of the plant,” says Dr. Lankester, “to be able, in the minute cells of which it is composed, to convert the carbonic acid and ammonia which it gets from the atmosphere into fibrine and albumen, and by easy chemical processes we can separate these substances from our vegetable food. Wheat, barley, oats, rye, rice, all contain fibrine, and some of them also albumen. Potatoes, cabbage, and asparagus contain albumen. It is a well-ascertained fact that those substances which contain most of these ‘nutritious secretions,’ as they have been called, support life the longest.” They change little during the process of digestion, entering the blood in a pure state, and being directly employed to renew the nervous and muscular matter which has been used up during work, either mental or muscular. Thus the supply of these substances is continually being drawn upon. The carbon, which forms their principal constituent, is converted into carbonic acid; and the nitrogen, which forms about a sixth part of their substance, re-appears in the nitrogen of urea, a substance which forms the principal solid constituent of the matter daily thrown from the system through the action of the kidneys. Thus the amount of urea which daily passes from the body affords a measure of the work done during the day. “This is not,” says Dr. Lankester, “the mere dream of the theorist; it has been practically demonstrated that increased stress upon the nervous system, viz., brain work, emotion, or excitement from disease, increases the quantity of urea and the demand

for nitrogenous food. In the same manner the amount of urea is the representative of the amount of muscular work done."

It has been calculated that the average amount of urea daily formed in the body of a healthy man is about 470 grains. To supply this daily consumption of nitrogenous matter, it is necessary that about four ounces of flesh-forming substance should be consumed daily. It is important, therefore, to inquire how this substance may be obtained. The requisite quantity of albuminous and fibrinous matter "is contained," says Dr. Lankester, "in a pound of beef; in two pounds of eggs; in two quarts of milk; in a pound of peas; in five pounds of rice; in sixteen pounds of potatoes; in two pounds of Indian meal; in a pound and a half of oatmeal; and in a pound and three-quarters of flour." A consideration of this list will show the importance of attending to the quality as well as the quantity of our food. A man of ordinary appetite might satisfy his hunger on potatoes or on rice, without by any means supplying his system with a sufficient amount of flesh-forming food. On the other hand, if a man were to live on bread and beef alone, he would load his system with an amount of nitrogenous food, although not taking what could be considered an excessive amount of daily nourishment. We see, also, how it is possible to continually vary the form in which we take the required supply of nitrogenous food, without varying the amount of that supply from day to day.

The supply itself should of course also vary from day to day as the amount of daily work may vary. What would be ample for a person performing a moderate amount of work would be insufficient for one who underwent daily great bodily or mental exertions, and would be too much for one who was taking holiday. It would appear, from the researches of Dr. Haughton, that the amount of urea daily formed in the body of a healthy man of average weight varies from 400 to 630 grains. Of this weight it appears that 300 grains results from the action of the internal organs. It

would seem, therefore, that the amount of flesh-forming food indicated in the preceding paragraph may be diminished in the proportion of 47 to 40 in the case of a person taking the minimum of exercise—that is, avoiding all movements save those absolutely necessary for comfort or convenience. On the other hand, that amount must be increased in the proportion of 74 to 63 in the case of a person (of average weight) working up to his full powers. It will be seen at once, therefore, that a hardworking man, whether labourer or thinker, must make good flesh-forming food constitute a considerable portion of his diet ; otherwise he would require to take an amount of food which would seriously interfere with his comfort and the due action of his digestive organs. For instance, if he lived on rice alone, he would require to ingest nearly seven pounds of food daily ; if on potatoes, he would require upwards of twenty-one pounds ; whereas one pound and a third of meat would suffice to supply the same amount of flesh-forming food.

Men who have to work, quickly find out what they require in the way of food. The Irishman who, while doing little work, will live contentedly on potatoes, asks for better flesh-forming food when engaged in heavy labour. In fact, the employer of the working man, so far from feeling aggrieved when his men require an improvement in their diet, either as respects quality or quantity, ought to look on the want as evidence that they are really working hard in his service, and also that they have a capacity for continuous work. The man who lives on less than the average share of flesh-forming food is doing less than an average amount of work ; the man who is unable to eat an average quantity of flesh-forming food, is *unable* to do an average amount of work. “ ‘On what principle do you discharge your men?’ I once said,” relates Dr. Lankester, “to a railway contractor. ‘Oh,’ he said, ‘it’s according to their appetites.’ ‘But,’ I said, ‘how do you judge of that?’ ‘Why,’ he said, ‘I send a clerk round when they are getting their dinners, and those who can’t eat he marks with a bit of chalk, and we send them about their business.’ ”

At a lecture delivered at the Royal Museum of Physics and Natural History at Florence, by Professor Mantegazza, a few years since, the Professor dwelt on the insufficient food which Italians are in the habit of taking, as among the most important causes of the weakness of the nation. "Italians," he said, "you should follow as closely as you can the example of the English in your eating and in your drinking, in the choice of flesh-meat (in tossing off bumpers of your rich wines),\* in the quality of your coffee, your tea, and your tobacco. I give you this advice, dear countrymen, not only as a medical man, but also as a patriot. It is quite evident, from the way millions of you perform the process which you call eating and drinking, that you have not the most elementary notions of the laws of physiology. You imagine that you are living. You are barely prolonging existence on maccaroni and water-melons. You neither know how to eat nor how to drink. You have no muscular energy; and, therefore, you have no continuous mental energy. The weakness of the individual, multiplied many millions of times, results in the collective weakness of the nation. Hence results insufficient work, and thence insufficient production. Thus the returns of the tax-collector and the custom-house officer are scanty, and the national exchequer suffers accordingly." Nor is all this, strange as it may sound, the mere gossip of the lecture-room. "The question of good feeding," says Dr. Lankester, "is one of national importance. It is vain to expect either brain or muscles to do efficient work when they are not provided with the proper material. Neither intellectual nor physical work can be done without good food."

We have now considered the two principal forms of food, the heat-forming—sometimes called the *amylaceous*—constituents, and the flesh-forming or *nitrogenous* constituents. But there are other substances which, although forming a smaller proportion of the daily food, are yet scarcely less

\* To this article of the Professor's faith decided objection must be taken, however.

important. Returning to our comparison of the human system to a steam-engine—we have seen how the heat-forming and flesh-forming constituents of food correspond to the supply of fuel and water; but an engine would quickly fall into a useless state if the wear and tear of the material of which it is constructed were not attended to and repaired. Now, in the human frame there are materials which are continually being used up, and which require to be continually restored, if the system is to continue free from disease. These materials are the mineral constituents of the system. Amongst them we must include *water*, which composes a much larger portion of our bodies than might be supposed. Seven-ninths of our weight consists simply of water. Every day there is a loss of about one-thirtieth part of this constituent of our system. The daily repair of this important waste of material is not effected by imbibing a corresponding supply of water. A large proportion of the weight of water daily lost is renewed in the solid food. Many vegetables consist principally of water. This is notably the case with potatoes. Where the water supplied to a district is bad, so that little water is consumed by the inhabitants—at least, without the addition of some other substance—it becomes important to notice the varying proportion of water present in different articles of food. As an instance of this, I may call attention to a remarkable circumstance observed during the failure of the potato crops in Ireland. Notwithstanding the great losses which the people sustained at that time, it was noticed that the amount of tea imported into Ireland exhibited a remarkable increase. This seemed at first sight a somewhat perplexing phenomenon. The explanation was recognized in the circumstance that the potato—a watery vegetable, as we have said—no longer formed the chief portion of the people's diet. Thus the deficiency in the supply of water had to be made up by the use of a larger quantity of fluid food; and as simple water was not palatable to the people, they drank tea in much larger quantities than they had been in the habit of taking before the famine.

But we have to consider the other mineral constituents of the system.

If I were to run through the list of all the minerals which exist within the body, I should weary the patience of the reader, and perhaps not add very much to the clearness of his ideas respecting the constitution of the human frame. Let it suffice to state generally that, according to the calculations of physiologists, a human body weighing 154 pounds contains about  $17\frac{1}{2}$  pounds of mineral matter; and that the most important mineral compounds existing within the body are those which contain lime, soda, and potash. Without pretending to any strictly scientific accuracy in the classification, we may say that the lime is principally found in the bones, the soda in the blood, the potash in the muscles; and according as one or other of these important constituents is wanting in our food, so will the corresponding portions of the frame be found to suffer.

We have a familiar illustration of the effects of unduly diminishing the supply of the mineral constituents of the body in the ravages which scurvy has worked amongst the crews of ships which have remained for a long period ill-supplied with fresh vegetables. Here it is chiefly the want of potash in the food which causes the mischief. An interesting instance of the rapid—almost startling—effects of food containing potash, in the cure of men stricken by scurvy, is related by Dana. The crew of a ship which had been several months at sea, but was now nearing the land, were prostrated by the ravages of scurvy. Nearly all seemed hopelessly ill. One young lad was apparently dying, the livid spots which were spreading over his limbs seeming to betoken his rapidly approaching end. At this moment a ship appeared in view which had but lately left the land, and was laden with fresh vegetables. Before long large quantities of the life-bearing food had been transferred to the decks of the other ship. The instincts of life taught the poor scurvy-stricken wretches to choose the vegetable which of all others was best suited to supply the want under which their frames

were wasting. They also were led by the same truthful instincts to prefer the raw to cooked vegetables. Thus the sick were to be seen eating raw onions with a greater relish than the gourmand shows for the most appetising viands. But the poor lad who was the worse of the sufferers had already lost the power of eating ; and it was without a hope of saving his life that some of his companions squeezed the juice of onions between his lips, already quivering with the tremor of approaching death. He swallowed a few drops, and presently asked for more. Shortly he began to revive, and to the amazement of all those who had seen the state of prostration to which he had been reduced, he regained in a few days his usual health and strength.

The elements which we require in order to supply the daily waste of the mineral constituents of the body are contained in greater or less quantities in nearly all the articles which man uses for food. But it may readily happen that, by adopting an ill-regulated diet, a man may not take a sufficient quantity of these important elements. It must also be noticed that articles of food, both animal and vegetable, may be deprived of a large proportion of their mineral elements by boiling ; and if, as often happens, the water in which the food has been boiled is thrown away, injurious effects can scarcely fail to result from the free use of food which has lost so important a portion of its constituent elements. Accordingly, when persons partake much of boiled meat, they should either consume the broth with the meat, or use it as soup on the alternate days. Vegetables steamed in small quantities of water (this water being taken with them), also afford a valuable addition to boiled meat. In fact, experience seems to have suggested the advantage of mixing carrots, parsnips, turnips, and greens with boiled meat ; but unfortunately the addition is not always made in a proper manner. If the vegetables are boiled separately in large quantities of water, and served up after this water has been thrown away, more harm than good is done by the addition ; since the appetite is satisfied with comparatively

useless food, instead of being left free to choose, as it might otherwise do, such forms of food as would best supply the requirements of the system. Salads and uncooked fruits, for instance, contain saline ingredients in large proportion, and could be used advantageously after a meal of boiled meat. Potatoes are likewise a valuable article of food on account of the mineral elements contained in them. And there can be no doubt that the value of potatoes as an article of food is largely increased when they are cooked in their skins, after the Irish fashion.

Lastly, we must consider those articles of food which promote the natural vital changes, but do not themselves come to form part of the frame, or, at least, not in any large proportion of their bulk. Such are tea, coffee, and cocoa ; alcoholic drinks ; narcotics ; and lastly, spices and condiments. We may compare the use of these articles of food to that of oil in lubricating various parts of a steam-engine. For, as the oil neither forms part of the heat-supply nor of the force-supply of the steam-engine, nor is used to replace the worn material of its structure, yet serves to render the movements of the machine more equable and effective, so the forms of food we are considering are neither heat-producing nor flesh-forming, nor do they serve to replace, to any great extent, the mineral constituents of the body, yet they produce a sense of refreshment accompanied with renewed vigour. It is difficult to determine in what precise way these effects are produced, but no doubt can exist as to the fact that they are really attributable to the forms of food to which we have assigned them.

Tea, coffee, and cocoa owe their influence on the nervous system to the presence of a substance which has received the various names of *theine*, *caffeine*, and *theobromine*. It is identical in composition with *piperine*, the most important ingredient in pepper. It may be separated in the form of delicate white, silky crystals, which have a bitter taste. In its concentrated form this substance is poisonous, and to this circumstance must be ascribed the ill effects which follow

from the too free use of strong tea or coffee. However, the instances of bad effects resulting from the use of "the cup which cheers but not inebriates" are few and far between, while the benefits derived from it are recognized by all. It has, indeed, been stated that no nation which has begun to make use of tea, coffee, or cocoa, has ever given up the practice; and no stronger evidence can be required of the value of those articles of food.

Of alcoholic liquors it is impossible to speak so favourably. They are made use of, indeed, almost as extensively as tea or coffee; they have been made the theme of the poet, and hailed as the emblems of all that is genial and convivial. Yet there can be little doubt that, when a balance is struck between the good and evil which have resulted to man from their use, the latter is found largely to preponderate. The consideration of these evils belongs, however, rather to the moralist than to the physiologist. I have here simply to consider alcoholic liquors as articles of food. There can be little doubt that, when used with caution and judgment, they afford in certain cases an important adjunct to those articles which are directly applied to the reparation of bodily waste. Without absolutely nourishing the frame, they ultimately lead to this end by encouraging the digestive processes which result in the assimilation of nutritive articles of food. But the quantity of alcohol necessary to effect this is far less than is usually taken even by persons who are termed temperate. It is also certain that hundreds make use of alcoholic liquors who have no necessity for them, and who would be better without them. Those who require them most are men who lead a studious sedentary life; and it is such men, also, who suffer most from excess in the use of alcoholic liquors.

It remains that I should make a few remarks on mistakes respecting the quantity of food.

Some persons fall into the habit of taking an excessive quantity of food, not from greediness, but from the idea that a large amount of food is necessary for the maintenance

of their strength. They thus overtax the digestive organs, and not only fail of their purpose, but weaken instead of strengthening the system. Especially serious is the mistake often made by persons in delicate health of swallowing—no other word can be used, for the digestive organs altogether refuse to respond to the action of the mouth—large quantities of some concentrated form of food, such as even the strongest stomach could not deal with in that form. I knew a person who, though suffering from weakness such as should have suggested the blandest and simplest forms of food, adopted as a suitable breakfast mutton-chops and bottled stout, arguing, when remonstrated with, that he required more support than persons in stronger health. He was simply requiring his weak digestive organs to accomplish work which would have taxed the digestive energies of the most stalwart labourer working daily in the open air for many hours.

On the other hand, a too abstemious diet is as erroneous in principle as a diet in excess of the natural requirements of the system. A diet which is simply too abstemious is perhaps less dangerous than persistent abstinence from the use of certain necessary forms of food. Nature generally prevents us from injuring ourselves by unwisely diminishing the quantity of food we take; but unfortunately she is not always equally decided in her admonitions respecting the quality of our food. A man may be injuring his health through a deficiency in the amount either of the heat-forming or of the flesh-forming food which he consumes, and yet know nothing of the origin of the mischief. It may also be noted that systematic abstinence, either as respects quantity or quality of food, is much more dangerous than an occasional fast. Indeed, it is not generally injurious either to abstain for several days from particular articles or forms of food, or to remain, for several hours beyond the usual interval between meals, without food of any sort. On the contrary, benefit often arises from each practice. The Emperor Aurelian used to attribute the good health he

enjoyed to his habit of abstaining for a whole day, once a month, from food of all sorts; and many have found the Lenten rules of abstinence beneficial. As a rule, however, change of diet is a safer measure than periodical fasting or abstinence from either heat-producing or flesh-forming food. It must be noticed, in conclusion, that young persons ought not, without medical advice, to fast or abstain for any length of time from the more important forms of food, as serious mischief to the digestive organs frequently follows from either course.

## *OZONE.*

THE singular gas termed ozone has attracted a large amount of attention from chemists and meteorologists. The vague ideas which were formed as to its nature when as yet it had been but newly discovered, have given place gradually to more definite views ; and though we cannot be said to have thoroughly mastered all the difficulties which this strange element presents, yet we know already much that is interesting and instructive.

Let us briefly consider the history of ozone.

Nine years after Priestley had discovered oxygen, Van Marum, the electrician, noticed that when electric sparks are taken through that gas, a peculiar odour is evolved. Most people know this odour, since it is always to be recognized in the neighbourhood of an electrical machine in action. In reality, it indicates the presence of ozone in the air. But for more than half a century after Van Marum had noticed it, it was supposed to be the “smell of electricity.”

In 1840, Schönbein began to inquire into the cause of this peculiar odour. He presently found that it is due to some change in the oxygen ; and that it can be produced in many ways. Of these, the simplest, and, in some respects, the most interesting, is the following :—“Take sticks of common phosphorus, scrape them until they have a metallic lustre, place them in this condition under a large bell-jar, and half-cover them with water. The air in the bell-jar is

soon charged with ozone, and a large room can readily be supplied with ozonized air by this process."

Schönbein set himself to inquire into the properties of this new gas, and very interesting results rewarded his researches. It became quite clear, to begin with, that whatever ozone may be, its properties are perfectly distinct from those of oxygen. Its power of oxidizing or rusting metals, for example, is much greater than that which oxygen possesses. Many metals which oxygen will not oxidize at all, even when they are at a high temperature, submit at once to the influence of ozone. But the power of ozone on other substances than metals is equally remarkable. Dr. Richardson states that, when air is so ozonized as to be only respirable for a short time, its destructive power is such that gutta-percha and india-rubber tubings are destroyed by merely conveying it.

The bleaching and disinfecting powers of ozone are very striking. Schönbein was at first led to associate them with the qualities of chlorine gas; but he soon found that they are perfectly distinct.

It had not yet been shown whether ozone was a simple or a compound gas. If simple, of course it could be but another form of oxygen. At first, however, the chances seemed against this view; and there were not wanting skilful chemists who asserted that ozone was a compound of the oxygen of the air with the hydrogen which forms an element of the aqueous vapour nearly always present in the atmosphere.

It was important to set this question at rest. This was accomplished by the labours of De la Rive and Marignac, who proved that ozone is simply another form of oxygen.

Here we touch on a difficult branch of modern chemical research. The chemical elements being recognized as the simplest forms of matter, it might be supposed that each element would be unchangeable in its nature. That a compound should admit of change, is of course a thing to be expected. If we decompose water, for instance, into its

component elements, oxygen and hydrogen, we may look on these gases as exhibiting water to us in another form. And a hundred instances of the sort might be adduced, in which, either by separating the elements of a compound, or by re-arranging them, we obtain new forms of matter without any real change of substance. But with an element, the case, one would suppose, should be different.

However, the physicist must take facts as he finds them ; and amongst the most thoroughly recognized chemical facts we have this one, that elementary substances may assume different forms. Chemists call the phenomenon allotropy. A well-known instance of allotropy is seen in red phosphorus. Phosphorus is one of the chemical elements ; and, as every one knows, the form in which it is usually obtained is that of a soft, yellow, semi-transparent solid, somewhat resembling bees' wax in consistence, poisonous, and readily taking fire. Red phosphorus is the same element, yet differs wholly in its properties. It is a powder, it does not readily take fire, and it is not poisonous.

Ozone, then, is another form of oxygen. It is the only instance yet discovered of gaseous allotropy.

And now we have to deal with the difficult and still-vexed questions of the way in which the change from oxygen is brought about, and the actual distinction between the two forms of the same gas. Schönbein held that common oxygen is produced by the combination of two special forms of oxygen—the positive and the negative, or, as he called them, ozone and ant ozone. He showed that, in certain conditions of the air, the atmospheric oxygen exhibits qualities which are the direct reverse of those which ozone exhibits, and are distinct from those of ordinary oxygen. In oxygen thus negatived or ant ozonized, animals cannot live any more than they can in nitrogen. The products of decomposition are not only not destroyed as by ozone, but seem subject to preservative influences, and speedily become singularly offensive ; dead animal matter rapidly putrefies, and wounds show a tendency to mortification.

But the theory of positive and negative forms of oxygen, though still held by a few physicists, has gradually given way before the advance of new and sounder modes of inquiry. It has been proved, in the first place, that ozone is denser than ordinary oxygen. The production of ozone is always followed by a contraction of the gas's volume, the contraction being greater or less according to the amount of oxygen which has been ozonized. Regularly as the observers—Messrs. Andrews and Tait—converted a definite proportion of oxygen into ozone, the corresponding contraction followed, and as regularly was the original volume of the gas restored when, by the action of heat, the ozone was reconverted into oxygen.

And now a very singular experiment was made by the observers, with results which proved utterly perplexing to them. Mercury has the power of absorbing ozone; and the experimenters thought that if, after producing a definite contraction by the formation of ozone, they could absorb the ozone by means of mercury, the quantity of oxygen which remained would serve to show them how much ozone had been formed, and thence, of course, they could determine the density of ozone.

Suppose, for instance, that we have one hundred cubic inches of oxygen, and that by any process we reduce it to a combination of oxygen and ozone occupying ninety-five cubic inches. Now, if the mercury absorbed the ozone, and we found, say, that there only remained eighty-five cubic inches of oxygen, we could reason in this way:—Ten cubic inches were occupied by the ozone before the mercury absorbed it; but these correspond to fifteen cubic inches of oxygen; hence, ozone must be denser than oxygen in the proportion of fifteen to ten, or three to two. And whatever result might have followed, a real absorption of the ozone by the mercury would have satisfactorily solved the problem.

But the result actually obtained did not admit of interpretation in this way. The apparent absorption of the

ozone by the mercury, that is, the disappearance of the ozone from the mixture, was accompanied by *no diminution of volume at all*. In other words, returning to our illustrative case, after the absorption of the ozone from the ninety-five cubic inches occupied by the mixture, there still remained ninety-five cubic inches of oxygen ; so that it seemed as though an evanescent volume of ozone corresponded in weight to five cubic inches of oxygen. This solution, of course, could not be admitted, since it made the density of ozone *infinite*.

The explanation of this perplexing experiment is full of interest and instruction. The following is the account given by Mr. C. W. Heaton (Professor of Chemistry at Charing Cross Hospital), slightly modified, however, so that it may be more readily understood.

Modern chemists adopt, as a convenient mode of representing the phenomena which gases exhibit, the theory that every gas, whether elementary or compound, consists of minute molecules. They suppose that these molecules are of equal size, and are separated by equal intervals so long as the gas remains unchanged in heat and density. This view serves to account for the features of resemblance presented by all gases. The features in which gases vary are accounted for by the theory that the molecules are differently constituted. The molecules are supposed to be clusters of atoms, and the qualities of a gas are assumed to depend on the nature and arrangement of these ultimate atoms. The molecules of some elements consist but of a single atom ; the molecules of others are formed by pairs of atoms ; those of others by triplets ; and so on. Again, the molecules of compound gases are supposed to consist of combinations of different *kinds* of atoms.

Now, Dr. Odling, to whom we owe the solution of the perplexing problem described above, thus interpreted the observed phenomena. A molecule of oxygen contains two atoms, one of ozone contains three, and the oxidizing power of ozone depends on the ease with which it parts with its third

*atom of oxygen.* Thus, in the experiment which perplexed Messrs. Andrews and Tait, the mercury only *seemed* to absorb the ozone; in reality it converted the ozone into oxygen by removing its third atom. And now we see how to interpret such a result as we considered in our illustrative case. Five cubic inches of oxygen gave up their atoms, each atom combining with one of the remaining oxygen doublets, so as to form a set of ozone triplets. Clearly, then, fifteen cubic inches of oxygen were transformed into ozone. They now occupied but ten cubic inches; so that the mixture, or ozonized oxygen, contained eighty-five cubic inches of oxygen and ten of ozone. When the mercury was introduced, it simply transformed all the ozone triplets into oxygen doublets, by taking away the odd atom from each. It thus left ten cubic inches of oxygen, which, with the remaining eighty-five, constituted the ninety-five cubic inches observed to remain after the supposed absorption of the ozone.

It follows, of course, that ozone is half as heavy again as oxygen.

But, as Mr. Heaton remarked, "this beautiful hypothesis, although accounting perfectly for all known facts, was yet but a probability. One link was lacking in the chain of evidence, and that link M. Soret has supplied by a happily devised experiment." Although mercury and most substances are only capable of converting ozone into oxygen, oil of turpentine has the power of absorbing ozone in its entirety. Thus, when the experiment was repeated, with oil of turpentine in place of the mercury, the ozone was absorbed, and the remaining oxygen, instead of occupying ninety-five inches, occupied but eighty-five. After this, no doubt could remain that Dr. Odling's ingeniously conceived hypothesis was the correct explanation of Messrs. Andrews and Tait's experiment.

We recognize, then, in ozone a sort of concentrated oxygen, with this peculiar property, that it possesses an extraordinary readiness to part with its characteristic third

atom, and so disappear *as ozone*, two-thirds of its weight remaining as oxygen.

It is to this peculiarity that ozone owes the properties which render it so important to our welfare. We are indeed, as yet, in no position to theorize respecting this element, our knowledge of its very existence being so recent, and our information respecting its presence in our atmosphere being of still more recent acquisition.

Indeed, it is well remarked by Mr. Heaton, that we had, until quite lately, no reason for confidently adopting Schönbein's view that ozone exists in our atmosphere. The test-papers which Schönbein made use of turned blue under the influence of ozone, it is true, but they were similarly influenced by other elements which are known to exist in our atmosphere, and even the sun's rays turned them blue. However, Dr. Andrews has shown how the character of the air producing the change can be further tested, so as to render it certain that ozone only has been at work. If air which colours the test-papers be found to lose the property after being heated, the change can only be due to ozone, because nitrous and nitric acids (which have the power of colouring the test-papers) would not be removed by the heat, whereas ozone is changed by heat into oxygen.

Once we are certain that ozone exists in the air, we must recognize the fact that its presence cannot fail to have an important bearing on our health and comfort; for ozone is an exceedingly active agent, and cannot exist anywhere without setting busily to its own proper work. What that work is, and whether it is beneficial or deleterious to ourselves, remains to be considered.

In the first place, ozone has immense power as a disinfectant. It decomposes the products emanating from putrefying matter more effectually than any other known element. Perhaps the most striking proof ever given of its qualities in this respect is that afforded by an experiment conducted by Dr. Richardson a few years ago.

He placed a pint of blood taken from an ox in a large

wide-mouthed bottle. The blood had then coagulated, and it was left exposed to the air until it had become entirely redissolved by the effects of decomposition. At the end of a year the blood was put into a stoppered bottle, and set aside for seven years. "The bottle was then taken from its hiding-place," says Dr. Richardson, "and an ounce of the blood was withdrawn. The fluid was so offensive as to produce nausea when the gases evolved from it were inhaled. It was subjected by Dr. Wood and myself to a current of ozone. For a few minutes the odour of ozone was destroyed by the odour of the gases from the blood; gradually the offensive smell passed away; then the fluid mass became quite sweet, and at last a faint odour of ozone was detected, whereupon the current was stopped. The blood was thus entirely deodorized; but another and most singular phenomenon was observed. The dead blood coagulated as the products of decomposition were removed, and this so perfectly, that from the new clot that was formed serum exuded. Before the experiment commenced, I had predicted on theoretical grounds that secondary coagulation would follow on purification; and this experiment, as well as several others afterwards performed, verified the truth of the prediction."

It will of course be understood that ozone, in thus acting as a disinfectant, is transformed into oxygen. It parts with its third atom as in the mercury experiment, and so loses its distinctive peculiarity. Thus we might be led to anticipate the results which come next to be considered.

Ozone has certain work to do, and in doing that work is transmuted into oxygen. It follows, then, that where there has been much work for ozone to do, there we shall find little ozone left in the air. Hence, in open spaces where there is little decomposing matter, we should expect to find more ozone than in towns or cities. This accords with what is actually observed. And not only is it found that country air contains more ozone than town air, but it is found that air which has come from the sea has more ozone than even

the country air, while air in the crowded parts of large cities has no ozone at all, nor has the air of inhabited rooms.

So far as we have gone, we might be disposed to speak unhesitatingly in favour of the effects produced by ozone. We see it purifying the air which would otherwise be loaded by the products of decomposing matter, we find it present in the sea air and the country air which we know to be so bracing and health-restoring after a long residence in town, and we find it absent just in those places which we look upon as most unhealthy.

Again, we find further evidence of the good effects of ozone in the fact that cholera and other epidemics never make their dreaded appearance in the land when the air is well supplied with ozone—or in what the meteorologists call “the ozone-periods.” And though we cannot yet explain the circumstance quite satisfactorily, we yet seem justified in ascribing to the purifying and disinfecting qualities of ozone our freedom at those times from epidemics to which cleanliness and good sanitary regulations are notedly inimical.

But there is a reverse side to the picture. And as we described an experiment illustrating the disinfecting qualities of ozone before describing the good effects of the element, we shall describe an experiment illustrating certain less pleasing qualities of ozone, before discussing the deleterious influences which it seems capable of exerting.

Dr. Richardson found that when the air of a room was so loaded with ozone as to be only respirable with difficulty, animals placed in the room were affected in a very singular manner. “In the first place,” he says, “all the symptoms of nasal catarrh and of irritation of the mucous membranes of the nose, the mouth, and the throat were rapidly induced. Then followed free secretion of saliva and profuse action of the skin—perspiration. The breathing was greatly quickened, and the action of the heart increased in proportion.” When the animals were suffered to remain yet longer within the room, congestion of the lungs followed,

and the disease called by physicians "congestive bronchitis" was set up.

A very singular circumstance was noticed also as to the effects of ozone on the different orders of animals. The above-mentioned effects, and others which accompanied them, the description of which would be out of place in these pages, were developed more freely in carnivorous than in herbivorous animals. Rats, for example, were much more easily influenced by ozone than rabbits were.

The results of Dr. Richardson's experiments prepare us to hear that ozone-periods, though characterized by the absence of certain diseases, bring with them their own forms of disease. Apoplexy, epilepsy, and other similar diseases seem peculiarly associated with the ozone-periods, insomuch that eighty per cent. of the deaths occurring from them take place on days when ozone is present in the air in larger quantities than usual. Catarrh, influenza, and affections of the bronchial tubes, also affect the ozone-periods.

We see, then, that we have much yet to learn respecting ozone before we can pronounce definitively whether it is more to be welcomed or dreaded. We must wait until the researches which are in progress have been carried out to their conclusion, and perhaps even then further modes of inquiry will have to be pursued before we can form a definite opinion.

## *DEW.*

THERE are few phenomena of common occurrence which have proved more perplexing to philosophers than those which attend the deposition of dew. Every one is familiar with these phenomena, and in very early times observant men had noticed them ; yet it is but quite recently that the true theory of dew has been put forward and established. This theory affords a striking evidence of the value of careful and systematic observation applied even to the simplest phenomena of nature.

It was observed, in very early times, that dew is only formed on clear nights, when, therefore, the stars are shining. It was natural, perhaps, though hardly philosophical, to conclude that dew is directly shed down upon the earth from the stars ; accordingly, we find the reference of dew to stellar influences among the earliest theories propounded in explanation of the phenomenon.

A theory somewhat less fanciful, but still depending on supposed stellar influences, was shortly put forward. It was observed that dew is only formed when the atmosphere is at a low temperature ; or, more correctly, when the air is at a much lower temperature than has prevailed during the daytime. Combining this peculiarity with the former ancient philosophers reasoned in the following manner : Cold generates dew, and dew appears only when the skies are clear—that is, when the stars are shining ; hence it follows that the stars generate cold, and thus lead indirectly

to the formation of dew. Hence arose the singular theory, that as the sun pours down heat upon the earth, so the stars (and also the moon and planets) pour down cold.

Nothing is more common—we may note in passing—than this method of philosophizing, especially in all that concerns weather-changes ; and perhaps it would be impossible to find a more signal instance of the mistakes into which men are likely to fall when they adopt this false method of reasoning ; for, so far is it from being true that the stars shed cold upon the earth, that the exact reverse is the case. It has been established by astronomers and physicists that an important portion of the earth's heat-supply is derived from the stars.

Following on these fanciful speculations came Aristotle's theory of dew—celebrated as one of the most remarkable instances of the approximation which may sometimes be made to the truth by clever reasoning on insufficient observations. For we must not fall into the mistake of supposing, as many have done, that Aristotle framed hypotheses without making observations ; indeed, there has seldom lived a philosopher who has made more observations than he did. His mistake was that he extended his observations too widely, not making enough on each subject. He imagined that, by a string of syllogisms, he could make a few supply the place of many observations.

Aristotle added two important facts to our knowledge respecting dew—namely, first, that dew is only formed in serene weather ; and secondly, that it is not formed on the summits of mountains. Modern observations show the more correct statement of the case to be that dew is *seldom* formed either in windy weather or on the tops of mountains. Now, Aristotle reasoned in a subtle and able manner on these two observations. He saw that dew must be the result of processes which are interfered with when the air is agitated, and which do not extend high above the earth's surface ; he conjectured, therefore, that dew is simply caused by the discharge of vapour from the air.

"Vapour is a mixture," he said, "of water and heat, and as long as water can get a supply of heat, vapour rises. But vapour cannot rise high, or the heat would get detached from it; and vapour cannot exist in windy weather, but becomes dissipated. Hence, in high places, and in windy weather, dew cannot be formed for want of vapour." He derided the notion that the stars and moon cause the precipitation of dew. "On the contrary, the sun," he said, "is the cause; since its heat raises the vapour, from which the dew is formed when that heat is no longer present to keep up the vapour."

Amidst much that is false, there is here a good deal that is sound. The notion that heat is some substance which floats up the vapour, and may become detached from it in high or windy places, is of course incorrect. So also is the supposition that the dew is produced by the *fall* of condensed vapour as the heat passes away. Nor is it correct to say that the absence of the sun causes the condensation of vapour, since, as we shall presently see, the cold which causes the deposition of dew results from more than the mere absence of the sun. But, in pointing out that the discharge of vapour from the air, owing to loss of heat, is the true cause of the deposition of dew, Aristotle expressed an important truth. It was when he attempted to account for the discharge that he failed. It will be observed, also, that his explanation does not account for the observed fact that dew is only formed in clear weather.

Aristotle's views did not find acceptance among the Greeks or Romans; they preferred to look on the moon, stars, and planets as the agents which cause the deposition of dew. "This notion," says a modern author, "was too beautiful for a Greek to give up, and the Romans could not do better than follow the example of their masters."

In the middle ages, despite the credit attached to Aristotle's name, those who cultivated the physical sciences were unwilling to accept his views; for the alchemists (who alone may be said to have been students of nature) founded

their hopes of success in the search for the philosopher's stone, the *elixir vitae*, and the other objects of their pursuit, on occult influences supposed to be exercised by the celestial bodies. It was unlikely, therefore, that they would willingly reject the ancient theory which ascribed dew to lunar and stellar radiations.

But at length Baptista Porta adduced evidence which justified him in denying positively that the moon or stars exercise any influence on the formation of dew. He discovered that dew is sometimes deposited on the inside of glass panes ; and again, that a bell-glass placed over a plant in cold weather is more copiously covered with dew within than without ; nay, he observed that even some opaque substances show dew on their *under* surface when none appears on the upper. Yet, singularly enough, Baptista Porta rejected that part of Aristotle's theory which was alone correct. He thought his observations justified him in looking on dew as condensed—not from vapour, as Aristotle thought—but from the air itself.

But now a new theory of dew began to be supported. We have seen that not only the believers in stellar influence, but Aristotle also, looked on dew as falling from above. Porta's experiments were opposed to this view. It seemed rather as if dew rose from the earth. Observation also showed that the amount of dew obtained at different heights from the ground diminishes with the height. Hence, the new theorists looked upon dew as an exhalation from the ground and from plants—a fine steam, as it were, rising upwards, and settling principally on the under surfaces of objects.

But this view, like the others, was destined to be overthrown. Muschenbroek, when engaged in a series of observations intended to establish the new view, made a discovery which has a very important bearing on the theory of dew : he found that, instead of being deposited with tolerable uniformity upon different substances,—as falling rain is, for instance, and as the rising rain imagined by the new

theorists ought to be,—dew forms very much more freely on some substances than on others.

Here was a difficulty which long perplexed physicists. It appeared that dew neither fell from the sky nor arose from the earth. The object itself on which the dew was formed seemed to play an important part in determining the amount of deposition.

At length it was suggested that Aristotle's long-neglected explanation might, with a slight change, account for the observed phenomena. The formation of dew was now looked upon as a discharge of vapour from the air, this discharge not taking place necessarily upwards or downwards, but always from the air next to the object. But it was easy to test this view. It was understood that the coldness of the object, as compared with the air, was a necessary element in the phenomenon. It followed, that if a cold object is suddenly brought into warm air, there ought to be a deposition of moisture upon the object. This was found to be the case. Any one can readily repeat the experiment. If a decanter of ice-cold water is brought into a warm room, in which the air is not dry—a crowded room, for example—the deposition of moisture is immediately detected by the clouding of the glass. But there is, in fact, a much simpler experiment. When we breathe, the moisture in the breath generally continues in the form of vapour. But if we breathe upon a window-pane, the vapour is immediately condensed, because the glass is considerably colder than the exhaled air.

But although this is the correct view, and though physicists had made a noteworthy advance in getting rid of erroneous notions, yet a theory of dew still remained to be formed; for it was not yet shown how the cold, which causes the deposition of dew, is itself occasioned. The remarkable effects of a clear sky and serene weather in encouraging the formation of dew, were also still unaccounted for. On the explanation of these and similar points, the chief interest of the subject depends. Science owes the elucidation of these difficulties to Dr. Wells, a London physician, who studied

the subject of dew in the commencement of the present century. His observations were made in a garden three miles from Blackfriars Bridge.

Wells exposed little bundles of wool, weighing, when dry, ten grains each, and determined by their increase in weight the amount of moisture which had been deposited upon them. At first, he confined himself to comparing the amount of moisture collected on different nights. He found that although it was an invariable rule that cloudy nights were unfavourable to the deposition of dew, yet that on some of the very clearest and most serene nights, less dew was collected than on other occasions. Hence it became evident that mere clearness was not the only circumstance which favoured the deposition of dew. In making these experiments, he was struck by results which appeared to be anomalous. He soon found that these anomalies were caused by any obstructions which hid the heavens from his wool-packs : such obstructions hindered the deposition of dew. He tried a crucial experiment. Having placed a board on four props, he laid a piece of wool *on* the board, and another *under* it. During a clear night, he found that the difference in the amount of dew deposited on the two pieces of wool was remarkable : the upper one gained fourteen grains in weight, the lower one gained only four grains. He made a little roof over one piece of wool, with a sheet of pasteboard ; and the increase of weight was reduced to two grains, while a piece of wool outside the roof gained no less than sixteen grains in weight.

Leaving these singular results unexplained for a while, Dr. Wells next proceeded to test the temperature near his wool-packs. He found that where dew is most copiously produced, there the temperature is lowest. Now, since it is quite clear that the deposition of dew was not the cause of the increased cold—for the condensation of vapour is a process *producing heat*—it became quite clear that the formation of dew is dependent on and proportional to the loss of heat.

And now Wells was approaching the solution of the problem he had set himself; for it followed from his observations, that such obstructions as the propped board and the pasteboard roof *kept in the heat*. It followed also, from the observed effects of clear skies, that clouds *keep in the heat*. Now, what sort of heat is that which is prevented from escaping by the interference of screens, whether material or vaporous? There are three processes by which heat is transmitted from one body to another,—these are, conduction, convection, and radiation. The first is the process by which objects in contact communicate their heat to each other, or by which the heat in one part of a body is gradually transmitted to another part. The second is the process by which heat is carried from one place to another by the absolute transmission of heated matter. The third is that process by which heat is spread out in all directions, in the same manner as light. A little consideration will show that the last process is that with which we are alone concerned; and this important result flows from Dr. Wells' experiments, that *the rate of the deposition of dew depends on the rate at which bodies part with their heat by radiation*. If the process of radiation is checked, dew is less copiously deposited, and *vice versâ*.

When we consider the case of heat accompanied by light, we understand readily enough that a screen may interfere with the emission of radiant heat. We use a fire-screen, for instance, with the object of producing just such an interference. But we are apt to forget that what is true of luminous heat is true also of that heat which every substance possesses. In fact, we do not meet with many instances in which the effect of screens in preventing the loss of obscure heat is very noteworthy. There are some, as the warmth of a green-house at night, and so on; but they pass unnoticed, or are misunderstood. It was in this way that the explanation of dew-phenomena had been so long delayed. The very law on which it is founded had been *practically applied*, while its meaning had not been recognized. “I had often

in the pride of half-knowledge," says Wells, "smiled at the means frequently employed by gardeners to protect tender plants from cold, as it appeared to me impossible that a thin mat, or any such flimsy substance, could prevent them from attaining the temperature of the atmosphere, by which alone I thought them liable to be injured. But when I had seen that bodies on the surface of the earth become, during a still and serene night, colder than the atmosphere, by radiating their heat to the heavens, I perceived immediately a just reason for the practice which I had before deemed useless."

And now all the facts which had before seemed obscure were accounted for. It had been noticed that metallic plates were often dry when grass or wood was copiously moistened. Now, we know that metals part unwillingly with their heat by radiation, and therefore the temperature of a metal plate exposed in the open air is considerably higher than that of a neighbouring piece of wood. For a similar reason, dew is more freely deposited on grass than on gravel. Glass, again, is a good radiator, so that dew is freely deposited on glass objects,—a circumstance which is very annoying to the telescopist. The remedy employed is founded on Wells' observations—a cylinder of tin or card, called a dew-cap, is made to project beyond the glass, and thus to act as a screen, and prevent radiation.

We can now also interpret the effects of a clear sky. Clouds act the part of screens, and check the emission of radiant heat from the earth. This fact has been noticed before, but misinterpreted, by Gilbert White of Selborne. "I have often observed," he says, "that cold seems to descend from above; for when a thermometer hangs abroad on a frosty night, the intervention of a cloud shall immediately raise the mercury ten degrees, and a clear sky shall again compel it to descend to its former gauge." Another singular mistake had been made with reference to the power which clouds possess of checking the emission of radiant heat. It had been observed that on moonlit nights the eyes are apt to suffer in a peculiar way, which has occasionally brought on

temporary blindness. This had been ascribed to the moon's influence, and the term moon-blindness had therefore been given to the affection. In reality, the moon has no more to do with this form of blindness than the stars have to do with the formation of dew. The absence of clouds from the air is the true cause of the mischief. There is no sufficient check to the radiation of heat from the eyeballs, and the consequent chill results in temporary loss of sight, and sometimes even in permanent injury.

Since clouds possess this important power, it is clear that while they are present in the air there can never be a copious formation of dew, which requires, as we have seen, a considerable fall in the temperature of the air around the place of deposition. When the air is clear, however, radiation proceeds rapidly, and therefore dew is freely formed.

But it might seem that since objects in the upper regions of the air part with their radiant heat more freely than objects on the ground, the former should be more copiously moistened with dew than the latter. That the fact is exactly the reverse is thus explained. The cold which is produced by the radiation of heat from objects high in the air is communicated to the surrounding air, which, growing heavier, descends towards the ground, its place being supplied by warmer air. Thus the object is prevented from reducing the air in its immediate neighbourhood to so low a temperature as would be attained if this process of circulation were checked. Hence, a concave vessel placed below an object high in air, would serve to increase the deposition of dew by preventing the transfer of the refrigerated air. We are not aware that the experiment has ever been tried, but undoubtedly it would have the effect we have described. An object on the ground grows cold more rapidly, because the neighbouring air cannot descend after being chilled, but continues in contact with the object; also cold air is continually descending from the neighbourhood of objects higher in air which are parting with their radiant heat, and the cold air thus descending takes the place of warmer air, whose neigh-

bourhood might otherwise tend to check the loss of heat in objects on the ground.

Here, also, we recognize the cause of the second peculiarity detected by Aristotle—namely, that dew is only formed copiously in serene weather. When there is wind, it is impossible that the refrigerated air around an object which is parting with its radiant heat, can remain long in contact with the object. Fresh air is continually supplying the place of the refrigerated air, and thus the object is prevented from growing so cold as it otherwise would.

In conclusion, we should wish to point out the important preservative influence exercised during the formation of dew. If the heat which is radiated from the earth, or from objects upon it, during a clear night, were not repaired in any way, the most serious injury would result to vegetation. For instance, if the sun raised no vapour during the day, so that when night came on the air was perfectly dry, and thus the radiant heat passed away into celestial space without compensation, not a single form of vegetation could retain its life during the bitter cold which would result. But consider what happens. The sun's heat, which has been partly used up during the day in supplying the air with aqueous vapour, is gradually given out as this vapour returns to the form of water. Thus the process of refrigeration is effectually checked, and vegetation is saved from destruction. There is something very beautiful in this. During the day, the sun seems to pour forth his heat with reckless profusion, yet all the while it is being silently stored up; during the night, again, the earth seems to be radiating her heat too rapidly into space, yet all the while a process is going on by which the loss of heat is adequately compensated. Every particle of dew which we brush from the blades of grass, as we take our morning rambles, is an evidence of the preservative action of nature.

## *THE LEVELLING POWER OF RAIN.*

IT has been recognized, ever since geology has become truly a science, that the two chief powers at work in remodelling the earth's surface, are fire and water. Of these powers one is in the main destructive, and the other preservative. Were it not for the earth's vulcanian energies, there can be no question that this world would long since have been rendered unfit for life,—at least of higher types than we recognize among sea creatures. For at all times igneous causes are at work, levelling the land, however slowly ; and this not only by the action of sea-waves at the border-line between land and water, but by the action of rain and flood over inland regions. Measuring the destructive action of water by what goes on in the lifetime of a man, or even during many successive generations, we might consider its effects very slight, even as on the other hand we might underrate the effects of the earth's internal fires, were we to limit our attention to the effects of upheaval and of depression (not less preservative in the long run) during a few hundreds or thousands of years. As Lyell has remarked in his "Principles of Geology," "our position as observers is essentially unfavourable when we endeavour to estimate the nature and magnitude of the changes now in progress. As dwellers on the land, we inhabit about a fourth part of the surface ; and that portion is almost exclusively a theatre of decay, and not of reproduction. We know, indeed, that new deposits are annually formed in seas and lakes, and that every year some

new igneous rocks are produced in the bowels of the earth, but we cannot watch the progress of their formation ; and as they are only present to our minds by the aid of reflection, it requires an effort both of the reason and the imagination to appreciate duly their importance." But that they are actually of extreme importance, that in fact all the most characteristic features of our earth at present are due to the steady action of these two causes, no geologist now doubts.

I propose now to consider one form in which the earth's aqueous energies effect the disintegration and destruction of the land. The sea destroys the land slowly but surely, by beating upon its shores and by washing away the fragments shaken down from cliffs and rocks, or the more finely divided matter abstracted from softer strata. In this work the sea is sometimes assisted by the other form of aqueous energy—the action of rain. But in the main, the sea is the destructive agent by which shore-lines are changed. The other way in which water works the destruction of the land affects the interior of land regions, or only affects the shore-line by removing earthy matter from the interior of continents to the mouths of great rivers, whence perhaps the action of the sea may carry it away to form shoals and sandbanks. I refer to the direct and indirect effects of the downfall of rain. All these effects, without a single exception, tend to level the surface of the earth. The mountain torrent whose colour betrays the admixture of earthy fragments is carrying those fragments from a higher to a lower level. The river owes its colour in like manner to earth which it is carrying down to the sea level. The flood deposits in valleys matter which has been withdrawn from hill slopes. Rainfall, acts, however, in other ways, and sometimes still more effectively. The soaked slopes of great hills give way, and great landslips occur. In winter the water which has drenched the land freezes, in freezing expands, and then the earth crumbles and is ready to be carried away by fresh rains ; or when dry, by the action even of the wind alone. Landslips, too, are brought about frequently in the way, which are even more

remarkable than those which are caused by the unaided action of heavy rainfalls.

The most energetic action of aqueous destructive forces is seen when water which has accumulated in the higher regions of some mountain district breaks its way through barriers which have long restrained it, and rushes through such channels as it can find or make for itself into valleys and plains at lower levels. Such catastrophes are fortunately not often witnessed in this country, nor when seen do they attain the same magnitude as in more mountainous countries. It would seem, indeed, as though they could attain very great proportions only in regions where a large extent of mountain surface lies above the snow-line. The reason why in such regions floods are much more destructive than elsewhere will readily be perceived if we consider the phenomena of one of these terrible catastrophes.

Take, for instance, the floods which inundated the plains of Martigny in 1818. Early in that year it was found that the entire valley of the Bagnes, one of the largest side-valleys of the great valley of the Rhône, above Geneva, had been converted into a lake through the damming up of a narrow outlet by avalanches of snow and ice from a loftier glacier overhanging the bed of the river Dranse. The temporary lake thus formed was no less than half a league in length, and more than 200 yards wide, its greatest depth exceeding 200 feet. The inhabitants perceived the terrible effects which must follow when the barrier burst, which it could not fail to do in the spring. They, therefore, cut a gallery 700 feet long through the ice, while as yet the water was at a moderate height. When the waters began to flow through this channel, their action widened and deepened it considerably. At length nearly half the contents of the lake were poured off. Unfortunately, as the heat of the weather increased, the middle of the barrier slowly melted away, until it became too weak to withstand the pressure of the vast mass of water. Suddenly it gave way; and so completely that all the water in the lake rushed out in half an hour. The effects of this tremendous

outrush of the imprisoned water were fearful. "In the course of their descent," says one account of the catastrophe, "the waters encountered several narrow gorges, and at each of these they rose to a great height, and then burst with new violence into the next basin, sweeping along forests, houses, bridges, and cultivated land." It is said by those who witnessed the passage of the flood at various parts of its course, that it resembled rather a moving mass of rock and mud than a stream of water. "Enormous masses of granite were torn out of the sides of the valleys, and whirled for hundreds of yards along the course of the flood." M. Escher the engineer tells us that a fragment thus whirled along was afterwards found to have a circumference of no less than sixty yards. "At first the water rushed on at a rate of more than a mile in three minutes, and the whole distance (forty-five miles) which separates the Valley of Bagne from the Lake of Geneva was traversed in little more than six hours. The bodies of persons who had been drowned in Martigny were found floating on the further side of the Lake of Geneva, near Vevey. Thousands of trees were torn up by the roots, and the ruins of buildings which had been overthrown by the flood were carried down beyond Martigny. In fact, the flood at this point was so high, that some of the houses in Martigny were filled with mud up to the second story."

It is to be noted respecting this remarkable flood, that its effects were greatly reduced in consequence of the efforts made by the inhabitants of the lower valleys to make an outlet for the imprisoned waters. It was calculated by M. Escher that the flood carried down 300,000 cubic feet of water every second, an outflow five times as great as that of the Rhine below Basle. But for the drawing off of the temporary lake, the flood, as Lyell remarks, would have approached in volume some of the largest rivers in Europe. "For several months after the *débâcle* of 1818," says Lyell, "the Dranse, having no settled channel, shifted its position continually from one side to the other of the valley, carrying away newly erected bridges, undermining houses, and

continuing to be charged with as large a quantity of earthy matter as the fluid could hold in suspension. I visited this valley four months after the flood, and was witness to the sweeping away of a bridge and the undermining of part of a house. The greater part of the ice-barrier was then standing, presenting vertical cliffs 150 feet high, like ravines in the lava-currents of Etna, or Auvergne, where they are intersected by rivers." It is worthy of special notice that inundations of similar or even greater destructiveness have occurred in the same region at former periods.

It is not, however, necessary for the destructive action of floods in mountain districts that ice and snow should assist, as in the Martigny flood. In October, 1868, the cantons of Tessin, Grisons, Uri, Valois, and St. Gall, suffered terribly from the direct effects of heavy rainfall. The St. Gothard, Splügen, and St. Bernardin routes were rendered impassable. In the former pass twenty-seven lives were lost, besides many horses and waggons of merchandise. On the three routes more than eighty persons in all perished. In the small village of Loderio alone, no less than fifty deaths occurred. The damage in Tessin was estimated at £40,000. In Uri and Valois large bridges were destroyed and carried away. Everything attested the levelling power of rain; a power which, when the rain is falling steadily on regions whence it as steadily flows away, we are apt to overlook.

It is not, however, necessary to go beyond our own country for evidence of the destructive action of water. We have had during the past few years very striking evidence in this respect, which need scarcely be referred to more particularly here, because it will be in the recollection of all our readers. Looking over the annals of the last half-century only, we find several cases in which the power of running water in carrying away heavy masses of matter has been strikingly shown. Consider, for instance, the effects of the flood in Aberdeenshire and the neighbouring counties, early in August, 1829. In the course of two days a great flood extended itself over "that part of the north-east of Scotland

which would be cut off by two lines drawn from the head of Loch Rannoch, one towards Inverness and the other to Stonehaven." The total length of various rivers in this region which were flooded amounted to between 500 and 600 miles. Their courses were marked everywhere by destroyed bridges, roads, buildings, and crops. Sir T. D. Lauder records "the destruction of thirty-eight bridges, and the entire obliteration of a great number of farms and hamlets. On the Nairn, a fragment of sandstone fourteen feet long by three feet wide and one foot thick, was carried about 200 yards down the river. Some new ravines were formed on the sides of mountains where no streams had previously flowed, and ancient river channels, which had never been filled from time immemorial, gave passage to a copious flood." But perhaps the most remarkable effect of these inundations was the entire destruction of the bridge over the Dee at Ballater. It consisted of five arches, spanning a waterway of 260 feet. The bridge was built of granite, the pier, resting on rolled pieces of granite and gneiss. We read that the different parts of this bridge were swept away in succession by the flood, the whole mass of masonry disappearing in the bed of the river. Mr. Farquharson states that on his own premises the river Don forced a mass of 400 or 500 tons of stones, many of them of 200 or 300 pounds' weight, up an inclined plane, rising six feet in eight or ten yards, and left them in a rectangular heap about three feet deep on a flat ground, the heap ending abruptly at its lower extremity." At first sight this looks like an action the reverse of that levelling action which we have here attributed to water. But in reality it indicates the intense energy of this action; which drawing heavy masses down along with swiftly flowing water, communicates to them so great a momentum, that on encountering in their course a rising slope, they are carried up its face and there left by the retreating flood. The rising of these masses no more indicates an inherent uplifting power in running water, than the ascent of a gently rising slope by a mass which has rolled

headlong down the steep side of a hill indicates an upward action exerted by the force of gravity.

Even small rivers, when greatly swollen by rain, exhibit great energy in removing heavy masses. Thus Lyell mentions that in August, 1827, the College, a small river which flows down a slight declivity from the eastern watershed of the Cheviot Hills, carried down several thousand tons' weight of gravel and sand to the plain of the Till. This little river also carried away a bridge then in process of building, "some of the arch stones of which, weighing from half to three-quarters of a ton each, were propelled two miles down the rivulet." "On the same occasion the current tore away from the abutment of a mill-dam a large block of green-stone porphyry, weighing nearly two tons, and transported it to a distance of nearly a quarter of a mile. Instances are related as occurring repeatedly, in which from 1000 to 3000 tons of gravel are in like manner removed by this streamlet to still greater distances in one day."

It may appear, however, to the reader that we have in such instances as these the illustration of destructive agencies which are of their very nature limited within very narrow areas. The torrent, or even the river, may wear out its bed or widen it, but nevertheless can hardly be regarded as modifying the aspect of the region through which it flows. Even in this respect, however, the destructive action of water is not nearly so limited as it might appear to be. Taking a few centuries or a few thousand years, no doubt, we can attribute to the action of rivers, whether in ordinary flow or in flood, little power of modifying the region which they drain. But taking that wider survey (in time) of fluvial work which modern science requires, dealing with this form of aqueous energy as we deal with the earth's vulcanian energies, we perceive that the effects of river action in the course of long periods of time are not limited to the course which at any given time a river may pursue. In carrying down material along its course to the sea, a river is not merely wearing down its own bed, but is so changing it that

in the course of time it will become unfit to drain the region through which it flows. Its bottom must of necessity become less inclined. Now although it will then be lower than at present, and therefore be then even more than now the place to which the water falling upon the region traversed by the river will naturally tend, it will no longer carry off that water with sufficient velocity. Three consequences will follow from this state of things. In the first place there will be great destruction in the surrounding region through floods because of inadequate outflow; in the second place, the overflowing waters will in the course of time find new channels, or in other words new rivers will be formed in this region; thirdly, owing to the constant presence of large quantities of water in the depressed bed of the old river, the banks on either side will suffer, great landslips occurring and choking up its now useless channel. Several rivers are undergoing these changes at the present time, and others, which are manifestly unfit for the work of draining the region through which they flow (a circumstance attested by the occurrence of floods in every wet season), must before long be modified in a similar way.

We are thus led to the consideration of the second form in which the destructive action of inland waters, or we may truly say, the destructive action of *rain*, is manifested,—viz., in landslips. These, of course, are also caused not unfrequently by vulcanian action, but equally of course landslips so caused do not belong to our present subject. Landslips caused directly or indirectly by rain, are often quite as extensive as those occasioned by vulcanian energy, and they are a great deal more common. We may cite as a remarkable instance a landslip of nearly half a mile in breadth, now in progress, in a district of the city of Bath called Hedgmead, which forms a portion of the slope of Beacon Hill. It is attributed to the action of a subterranean stream on a bed of gravel, the continued washing away of which causes the shifting; but the heavy rains of 1876-77 caused the landslip to become much more marked.

Besides slow landslips, however, rain not unfrequently causes great masses of earth to be precipitated suddenly, and where such masses fall into the bed of a river, local deluges of great extent and of the most destructive character often follow. The following instances, cited in an abridged form from the pages of Lyell's "Principles of Geology," attest the terrible nature of catastrophes such as these.

Two dry seasons in the White Mountains of New Hampshire were followed by heavy rains on August 28, 1826. From the steep and lofty slopes of the River Saco great masses of rock and stone were detached, and descending carried along with them "in one promiscuous and frightful ruin, forests, shrubs, and the earth which sustained them." "Although there are numerous indications on the steep sides of these hills of former slides of the same kind, yet no tradition had been handed down of any similar catastrophe within the memory of man, and the growth of the forest on the very spots now devastated clearly showed that for a long interval nothing similar had occurred. One of these moving masses was afterwards found to have slid three miles, with an average breadth of a quarter of a mile." At the base of the vast chasms formed by these natural excavations, a confused mass of ruins was seen, consisting of transported earth, gravel, rocks, and trees. Forests were prostrated with as much ease as if they had been mere fields of grain; if they resisted for a while, "the torrent of mud and rock accumulated behind till it gathered sufficient force to burst the temporary barrier." "The valleys of the Amonoosuck and Saco presented, for many miles, an uninterrupted scene of desolation, all the bridges being carried away, as well as those over the tributary streams. In some places the road was excavated to the depth of from fifteen to twenty feet; in others it was covered with earth, rocks, and trees to as great a height. The water flowed for many weeks after the flood, as densely charged with earth as it could be without being changed into mud, and marks were seen in various localities of its having risen on either side of the

valley to more than twenty-five feet above the ordinary level." But perhaps the most remarkable evidence of the tremendous nature of this cataclysm is to be found in Lyell's statements respecting the condition of the region nineteen years later. "I found the signs of devastation still very striking," he says; "I also particularly remarked that the surface of the bare granite rocks had been smoothed by the passage over them of so much mud and stone." Professor Hubbard mentions in *Silliman's Journal* that "in 1838 the deep channels worn by the avalanches of mud and stone, and the immense heaps of boulders and blocks of granite in the river channel, still formed a picturesque feature in the scenery."

It will readily be understood that when destruction such as this follows from landslips along the borders of insignificant rivers, those occurring on the banks of the mighty rivers which drain whole continents are still more terrible. The following account from the pen of Mr. Bates the naturalist, indicates the nature of the landslips which occur on the banks of the Amazon. "I was awoke before sunrise, one morning," he says, "by an unusual sound resembling the roar of artillery; the noise came from a considerable distance, one crash succeeding another. I supposed it to be an earthquake, for, although the night was breathlessly calm, the broad river was much agitated, and the vessel rolled heavily. Soon afterwards another loud explosion took place, followed by others which lasted for an hour till the day dawned, and we then saw the work of destruction going forward on the other side of the river, about three miles off. Large masses of forest, including trees of colossal size, probably 200 feet in height, were rocking to and fro, and falling headlong one after another into the water. After each avalanche the wave which it caused returned on the crumbly bank with tremendous force, and caused the fall of other masses by undermining. The line of coast over which the landslip extended was a mile or two in length; the end of it, however, was hid from our view by an inter-

venering island. It was a grand sight ; each downfall created a cloud of spray ; the concussion in one place causing other masses to give way a long distance from it, and thus the crashes continued, swaying to and fro, with little prospect of termination. When we glided out of sight two hours after sunrise the destruction was still going on."

We might consider here the action of glaciers in gradually grinding down the mountain slopes, the destructive action of avalanches, and a number of other forms in which snow and ice break down by slow degrees the upraised portions of the earth. For in reality all these forms of destructive action take their origin in the same process whence running waters and heavy rainfalls derive their power. All these destructive agencies are derived from the vapour of water in the air. But it seems better to limit the reader's attention in this place to the action of water in the liquid form ; and therefore we proceed to consider the other ways in which rain wears down the land.

Hitherto we have considered effects which are produced chiefly along the courses of rivers, or in their neighbourhood. But heavy rainfall acts, and perhaps in the long run as effectively (when we remember the far wider region affected) over wide tracts of nearly level ground, as along the banks of torrents and rivers.

The rain which falls on plains or gently undulating surfaces, although after a while it dries up, yet to some degree aids in levelling the land, partly by washing down particles of earth, however slowly, to lower levels, partly by soaking the earth and preparing a thin stratum of its upper surface to be converted into dust, and blown away by the wind. But it is when very heavy storms occur that the levelling action of rain over widely extending regions can be most readily recognized. Of this fact observant travellers cannot fail to have had occasional evidence. Sir Charles Lyell mentions one instance observed by him, which is specially interesting. "During a tour in Spain," he says, "I was surprised to see a district of gently undulating

ground in Catalonia, consisting of red and grey sandstone, and in some parts of red marl, almost entirely denuded of herbage, while the roots of the pines, holm oaks, and some other trees, were half exposed, as if the soil had been washed away by a flood. Such is the state of the forests, for example, between Oristo and Vich, and near San Lorenzo. But being overtaken by a violent thunderstorm, in the month of August, I saw the whole surface, even the highest levels of some flat-topped hills, streaming with mud, while on every declivity the devastation of torrents was terrific. The peculiarities in the physiognomy of the district were at once explained, and I was taught that, in speculating on the greater effects which the direct action of rain may once have produced on the surface of certain parts of England, we need not revert to periods when the heat of the climate was tropical." He might have cited instances of such storms occurring in England. For example, White, in his delightful "*Natural History of Selborne*," describes thus the effects of a storm which occurred on June 5, 1784: "At about a quarter after two the storm began in the parish of Harpley, moving slowly from north to south, and from thence it came over Norton Farm and so to Grange Farm, both in this parish. Had it been as extensive as it was violent (for it was very short) it must have ravaged all the neighbourhood. The extent of the storm was about two miles in length and one in breadth. There fell prodigious torrents of rain on the farms above mentioned, which occasioned a flood as violent as it was sudden, doing great damage to the meadows and fallows by deluging the one and washing away the soil of the other. The hollow lane towards Alton was so torn and disordered as not to be passable till mended, rocks being removed which weighed two hundredweight."

We have mentioned the formation of dust, and the action of wind upon it, as a cause tending to level the surface of the land. It may appear to many that this cause is too insignificant to be noticed among those which modify the earth's

surface. In reality, however, owing to its continuous action, and to its always acting (in the main) in one direction, this cause is much more important than might be supposed. We overlook its action as actually going on around us, because in a few years, or in a few generations, it produces no change that can be readily noticed. But in long periods of time it changes very markedly the level of lower lands, and that too even in cities, where means exist for removing the accumulations of dust which are continually collecting on the surface of the earth. We know that the remains of old Roman roads, walls, houses, and so forth, in this country, are found, not at the present level of the surface, but several feet—in some cases many yards—below this level. The same holds elsewhere, under the same conditions—that is, where we know quite certainly that the substances thus found underground were originally on the surface, and that there has been neither any disturbance causing them to be engulfed, nor any deposition of scoriæ, volcanic dust, or other products of subterranean disturbance. We cannot hesitate to regard this burying of old buildings as due to the continual deposition of dust, which eventually becomes compacted into solid earth. We know, moreover, that the formation of dust is in the main due to rain converting the surface layers of the earth into mud, which on drying requires but the frictional action of heavy winds to rise in clouds of dust. In some soils this process goes on more rapidly than in others, as every one who has travelled much afoot is aware. There are parts of England, for instance, where, even in the driest summer, the daily deposition of dust on dry and breezy days is but slight, others where in such weather a dust layer at least a quarter of an inch in thickness is deposited in the course of a day. If we assumed, which would scarcely seem an exaggerated estimate, that in the course of a single year a layer of dust averaging an inch in thickness is deposited over the lower levels of the surface of the land, we should find that the average depth of the layer formed in the last thousand years would amount to no

less than eighty-three feet. Of course in inhabited places the deposition of dust is checked, though not so much as most persons imagine. There is not probably in this country a single building five hundred years old, originally built at a moderately low level, the position of whose foundation does not attest the constant gathering of matter upon the surface. The actual amount by which the lower levels are raised and the higher levels diminished in the course of a thousand years may be very much less, but that it must amount to many feet can scarcely be questioned.

And as in considering the action of rain falling over a wide range of country, we have to distinguish between the slow but steady action of ordinary rains and the occasional violent action of great storms of rain, so in considering the effects of drought following after rain which has well saturated the land we have to distinguish between ordinarily dusty times and occasions when in a very short time, owing to the intensity of the heat and the violence of the wind large quantities of dust are spread over a wide area. Darwin thus describes the effect of such exceptional drought, as experienced in the years 1827-1832 in Buenos Ayres :—"So little rain fell that the vegetation, even to the thistles, failed ; the brooks were dried up, and the whole country assumed the appearance of a dusty high road. This was especially the case in the northern part of the province of Buenos Ayres, and the southern part of Santa Fé." He describes the loss of life caused by the want of water, and many remarkable circumstances of the drought which do not here specially concern us. He then goes on to speak of the dust which gathered over the open country. "Sir Woodbine Parish," he says, "informed me of a very curious source of dispute. The ground being so long dry, such quantities of dust were blown about that in this open country the landmarks became obliterated, and people could not tell the limits of their estates." The dust thus scattered over the land, whether left or removed, necessarily formed part of the solid material brought from higher to lower levels, indirectly (in this case)

through the action of rain; for a drought can only convert into friable matter earth which has before been thoroughly soaked. But the action of rain, which had originally led to the formation of these enormous masses of dust, presently took part in carrying the dust in the form of mud to yet lower levels. "Subsequently to the drought of 1827 to 1832," proceeds Darwin, "a very rainy season followed, which caused great floods. Hence it is almost certain that some thousands of the skeletons" (of creatures whose deaths he had described before) "were buried by the deposits of the very next year. What could be the opinion of a geologist, viewing such an enormous collection of bones, of all kinds of animals and of all ages, thus embedded in one thick earthy mass? Would he not attribute it to a flood having swept over the surface of the land, rather than to the common order of things?" In fact, a single great drought, followed by a very rainy season, must in this instance, which was however altogether exceptional, have produced a layer or stratum such as geologists would ordinarily regard as the work of a much longer time and much more potent disturbing causes.

It may be well to consider in this place the question whether in reality the quantity of rain which falls now during our winter months does not greatly exceed that which formerly fell in that part of the year. The idea is very prevalent that our winters have changed entirely in character in recent times, and the fear (or the hope?) is entertained that the change may continue in the same direction until wet and mild winters replace altogether the cold which prevailed in former years. There is no sufficient reason, however, for supposing that any such change is taking place. It is, indeed, not difficult to find in the meteorological annals of the first half of the present century, instances of the occurrence of several successive winters very unlike the greater number of those which we have experienced during the last ten or twelve years. But if we take any considerable series of years in the last century we find the alternations of the

weather very similar to those we at present recognize. Consider, for instance, Gilbert White's brief summary of the weather from 1768 onwards :—

For the winter of 1768-69 we have October and the first part of November rainy; thence to the end of 1768 alternate rains and frosts; January and February frosty and rainy, with gleams of fine weather; to the middle of March, wind and rain.

For the winter of 1769-70 we have October frosty, the next fortnight rainy, the next dry and frosty. December windy, with rain and intervals of frost (the first fortnight very foggy); the first half of January frosty, thence to the end of February mild hazy weather. March frosty and brighter.

For 1770-71, from the middle of October to the end of the year, almost incessant rains; January severe frosts till the last week, the next fortnight rain and snow, and spring weather to the end of February. March frosty.

For 1771-72, October rainy, November frost with intervals of fog and rain, December bright mild weather with hoar frosts; then six weeks of frost and snow, followed by six of frost, sleet, hail, and snow.

For 1772-73, October, November, and to December 22, rain, with mild weather; to the end of 1772, cold foggy weather; then a week of frost, followed by three of dark rainy weather. First fortnight of February frost; thence to the end of March misty showery weather.

Passing over the winter of 1773-74, which was half rainy, half frosty, what could more closely resemble the winter weather we have had so much of during the last few years, than that experienced in the winter of 1774-75? From August 24 to the third week of November, there was rain, with frequent intervals of sunny weather; to the end of December, dark dripping fogs; to the end of the first fortnight in March, rain almost every day.

And so on, with no remarkable changes, until the year 1792, the last of Gilbert White's records.

If we limit our attention to any given month of winter, we find the same mixture of cold and dry with wet and open weather as we are familiar with at present. Take, for instance, the month usually the most wintry of all, viz., January. Passing over the years already considered, we have January, 1776, dark and frosty with much snow till the 26th (at this time the Thames was frozen over), then foggy with hoar frost; January, 1777, frosty till the 10th, then foggy and showery; 1778, frosty till the 13th, then rainy to the 24th, then hard frost; 1779, frost and showers throughout January; 1780, frost throughout; 1781, frost and snow to the 25th, then rain and snow; 1782, open and mild; 1783, rainy with heavy winds; 1784, hard frost; 1785, a thaw on the 2nd, then rainy weather to the 28th, the rest of the month frosty; 1786, frost and snow till January 7, then a week mild with much rain, the next week heavy snow, and the rest mild with frequent rain; 1787, first twenty-four days dark moist mild weather, then four days frost, the rest mild and showery; 1788, thirteen days mild and wet, five days of frost, and from January 18 to the end of the month dry windy weather; 1789, thirteen days hard frost, the rest of the month mild with showers; 1790, sixteen days of mild foggy weather with occasional rain, to the 21st frost, to the 28th dark with driving rains, and the rest mild dry weather; 1791, the whole of January mild with heavy rains; and lastly 1792, "some hard frost in January, but mostly wet and mild."

There is nothing certainly in this record to suggest that any material change has taken place in our January weather during the last eight years. And if we had given the record of the entire winter for each of the years above dealt with the result would have been the same.

We have, in fact, very striking evidence in Gilbert White's account of the cold weather of December, 1784, which he specially describes as "very extraordinary," to show that neither our severe nor our average winter weather can differ materially from that which people experienced

in the eighteenth century. "In the evening of December 9," he says, "the air began to be so very sharp that we thought it would be curious to attend to the motions of a thermometer; we therefore hung out two, one made by Martin and one by Dolland" (*sic*, presumably Dollond), "which soon began to show us what we were to expect; for by ten o'clock they fell to twenty-one, and at eleven to four, when we went to bed. On the 10th, in the morning the quicksilver in Dolland's glass was down to half a degree below zero, and that of Martin's, which was absurdly graduated only to four degrees above zero, sank quite into the brass guard of the ball, so that when the weather became most interesting this was useless. On the 10th, at eleven at night, though the air was perfectly still, Dolland's glass went down to one degree below zero!" The note of exclamation is White's. He goes on to speak of "this strange severity of the weather," which was not exceeded that winter, or at any time during the twenty-four years of White's observations. Within the last quarter of a century, the thermometer, on more than one occasion, has shown two or three degrees below zero. Certainly the winters cannot be supposed to have been ordinarily severer than ours in the latter half of the last century, when we find that thermometers, by well-known instrument makers, were so constructed as to indicate no lower temperature than four degrees above zero.

Let us return, after this somewhat long digression, to the levelling action of rain and rivers.

If we consider this action alone, we cannot but recognize in it a cause sufficient to effect the removal of all the higher parts of the land to low levels, and eventually of all the low-lying land to the sea, in the course of such periods as geology makes us acquainted with. The mud-banks at the mouths of rivers show only a part of what rain and river action is doing, yet consider how enormous is the mass which is thus carried into the sea. It has been calculated that in a single week the Ganges alone carries away from the

soil of India and delivers into the sea twice as much solid substance as is contained in the great pyramid of Egypt. "The Irrawaddy," says Sir J. Herschel, "sweeps off from Burmah 62 cubit feet of earth in every second of time on an average, and there are 86,400 seconds in every day, and 365 days in every year; and so on for other rivers. Nor is there any reason to fear or hope that the rains will cease, and this destructive process come to an end. For though the quantity of water on the surface of the earth is probably undergoing a slow process of diminution, small portions of it year by year taking their place as waters under the earth,\* yet these processes are far too slow to appreciably affect the supply of water till a far longer period has elapsed than that during which (in all probability) life can continue upon the earth..

When we consider the force really represented by the downfall of rain, we need not greatly wonder that the

\* Those whose custom it is to regard all theorizing respecting the circumstances revealed by observation as unscientific, may read with profit an extremely speculative passage in Newton's *Principia* relating to the probable drying up of the earth in future ages. "As the seas," he says, "are absolutely necessary to the constitution of our earth, that from them the sun, by its heat, may exhale a sufficient quantity of vapours, which, being gathered together into clouds, may drop down in rain, for watering of the earth, and for the production and nourishment of vegetables; or being condensed with cold on the tops of mountains (as some philosophers with reason judge), may run down in springs and rivers; so for the conservation of the seas and fluids of the planets, comets seem to be required, that, from their exhalations and vapours condensed, the wastes of the planetary fluids spent upon vegetation and putrefaction, and converted into dry earth, may be ultimately supplied and made up; for all vegetables entirely derive their growths from fluids, and afterwards, in great measure, are turned into dry earth by putrefaction; and a sort of slime is always found to settle at the bottom of putrefied fluids; and hence it is that the bulk of the solid earth is continually increased; and the fluids, if they are not supplied from without, must be in a continual decrease, and quite fail at last. I suspect, moreover, that it is chiefly from the comets that spirit comes which is indeed the smallest but the most subtle and useful part of our air, and so much required to sustain the life of all things with us."

levelling power of rain is so effective. The sun's heat is the true agent in thus levelling the earth, and if we regard, as we justly may, the action of water, whether in the form of rain or river, or of sea-wave raised by wind or tide, as the chief levelling and therefore destructive force at work upon the earth, and the action of the earth's vulcanian energies as the chief restorative agent, then we may fairly consider the contest as lying between the sun's heat and the earth's internal heat. There can be little question as to what would be the ultimate issue of the contest if land and sea and air all endured or were only so far modified as they were affected by these causes. Sun-heat would inevitably prevail in the long run over earth-heat. But we see from the condition of our moon how the withdrawal of water and air from the scene must diminish the sun's power of levelling the irregularities of the earth's surface. We say advisedly *diminish*, not *destroy*; for there can be no question that the solar heat alternating with the cold of the long lunar night is still at work levelling, however slowly, the moon's surface; and the same will be the case with our earth when her oceans and atmosphere have disappeared by slow processes of absorption.

The power actually at work at present in producing rain, and so, indirectly, in levelling the earth's surface, is enormous. I have shown that the amount of heat required to evaporate a quantity of water which would cover an area of 100 square miles to a depth of one inch would be equal to the heat which would be produced by the combustion of half a million tons of coals, and that the amount of force of which this consumption of heat would be the equivalent corresponds to that which would be required to raise a weight of upwards of one thousand millions of tons to a height of one mile.\* When we remember that the land surface of the earth amounts to about fifty millions of square miles, we perceive how enormous must be the force-equivalent of the annual rainfall of our earth. We are apt to

\* See my "Science Byways," pp. 244, 245.

overlook when contemplating the silent and seemingly quiet processes of nature—such as the formation of the rain-cloud or the precipitation of rain—the tremendous energy of the forces really causing these processes. “I have seen,” says Professor Tyndall, “the wild stone-avalanches of the Alps, which smoke and thunder down the declivities with a vehemence almost sufficient to stun the observer. I have also seen snow-flakes descending so softly as not to hurt the fragile spangles of which they were composed ; yet to produce from aqueous vapour a quantity which a child could carry of that tender material demands an exertion of energy competent to gather up the shattered blocks of the largest stone-avalanche I have ever seen, and pitch them to twice the height from which they fell.”

## *ANCIENT BABYLONIAN ASTROGONY.*

IT is singular to consider how short a time elapsed, after writings in the arrow-headed or cuneiform letters (the Keilschriften of the Germans) were discovered, before, first, the power of interpreting them was obtained, and, secondly, the range of the cuneiform literature (so to speak) was recognized. Not more than ninety years have passed since the first specimens of arrow-headed inscriptions reached Europe. They had been known for a considerable time before this. Indeed, it has been supposed that the Assyrian letters referred to by Herodotus, Thucydides, and Pliny, were in this character. Della Valle and Figueroa, early in the seventeenth century, described inscriptions in arrow-headed letters, and hazarded the idea that they are to be read from left to right. But no very satisfactory evidence was advanced to show whether the inscriptions were to be so read, or from right to left, or, as Chardin suggested, in vertical lines. The celebrated Olaus Gerhard Tychsen, of Rostock, and other German philologists, endeavoured to decipher the specimens which reached Europe towards the end of the last century; but their efforts, though ingenious and zealous, were not rewarded with success. In 1801 Dr. Hager advanced the suggestion that the combinations formed by the arrow-heads did not represent letters but words, if not entire sentences. Lichtenstein, on the other hand, maintained that the letters belonged to an old form of the Arabic or Coptic character;

and he succeeded to his own satisfaction in finding various passages from the Koran in the cuneiform inscriptions. Dr. Grotfend was the first to achieve any real success in this line of research. It is said that he was led to take up the subject by a slight dispute with one of his friends, which led to a wager that he would decipher one of the cuneiform inscriptions. The results of his investigations were that cuneiform inscriptions are alphabetical, not hieroglyphical ; that the language employed is the basis of most of the Eastern languages ; and that it is written from right to left. Since his time, through the labours of Rich, Botta, Rawlinson, Hincks, De Saulcy, Layard, Sayce, George Smith, and others, the collection and interpretation of the arrow-headed inscriptions have been carried out with great success. We find reason to believe that, though the original literature of Babylon was lost, the tablet libraries of Assyria contained copies of most of the writings of the more ancient nation. Amongst these have been found the now celebrated descriptions of the Creation, the Fall of Man, the Deluge, the Tower of Babel, and other matters found in an abridged and expurgated form in the book of Genesis. It is to that portion of the Babylonian account which relates to the creation of the sun and moon and stars that I wish here to call attention. It is not only curious in itself, but throws light, in my opinion, on questions of considerable interest connected with the views of ancient Eastern nations respecting the heavenly bodies.

It may be well, before considering the passage in question, to consider briefly—though we may not be able definitely to determine—the real antiquity of the Babylonian account.

In Smith's interesting work on the Chaldaean account of Genesis, the question whether the Babylonian account preceded the writing of the book of Genesis, or *vice versa*, is not definitely dealt with. Probably this part of his subject was included among the "important comparisons and conclusions with respect to Genesis" which he preferred to

avoid, as his “desire was first to obtain the recognition of the evidence without prejudice.” It might certainly have interfered to some degree with the unprejudiced recognition of the evidence of the tablets if it had been maintained by him, and still more if he had demonstrated, that the Babylonian is the earlier version. For the account in the book of Genesis, coming thus to be regarded as merely an expurgated version of a narrative originally containing much fabulous matter, and not a little that is monstrous and preposterous, would certainly not have been presented to us in quite that aspect in which it had long been regarded by theologians.

But although Mr. Smith states that he placed the various dates as low as he fairly could, considering the evidence,—nay, that he “aimed to do this rather than to establish any system of chronology,”—there can be no mistake about the relative antiquity which he in reality assigns to the Babylonian inscriptions. He states, indeed, that every copy of the Genesis legends belongs to the reign of Assurbanipal, who reigned over Assyria B.C. 670. But it is “acknowledged on all hands that the tablets are not the originals, but are only copies from earlier texts.” The Assyrians acknowledge themselves that this literature was borrowed from Babylonian sources, and of course it is to Babylonia we have to look to ascertain the approximate dates of the original documents “The difficulty,” he proceeds, “is increased by the following considerations : it appears that at an early period in Babylonian history a great literary development took place, and numerous works were produced which embodied the prevailing myths, religion, and science of that day. Written, many of them, in a noble style of poetry on one side, or registering the highest efforts of their science on the other, these texts became the standards for Babylonian literature, and later generations were content to copy these writings instead of making new works for themselves. Clay, the material on which they were written, was everywhere abundant, copies were multiplied, and by the veneration in which they were held these texts fixed and stereotyped the

style of Babylonian literature, and the language in which they were written remained the classical style in the country down to the Persian conquest. Thus it happens that texts of Rim-agu, Sargon, and Hammurabi, who were 1000 years before Nebuchadnezzar and Nabonidus, show the same language as the texts of these later kings, there being no sensible difference in style to match the long interval between them,—precisely as a certain devotional style of writing of our own day closely resembles the style of the sixteenth century.

We cannot, then, from the style, determine the age of the original writings from which the Assyrian tablets were copied. But there are certain facts which enable us to form an opinion on this point. Babylonia was conquered about B.C. 1300, by Tugultiminip, king of Assyria. For 250 years before that date a foreign race (called by Berosus, Arabs) had ruled in Babylonia. There is no evidence of any of the original Babylonian Genesis tablets being written after the date of Hammurabi, under whom it is supposed that this race obtained dominion in Babylonia. Many scholars, indeed, regard Hammurabi as much more ancient; but none set him later than 1550 B.C.

Now, before the time of Hammurabi several races of kings reigned, their reigns ranging over a period of 500 years. They were called chiefly Kings of Sumir and Akkad—that is, Kings of Upper and Lower Babylonia. It is believed that before this period,—ranging, say, from about 2000 B.C. to 1550 B.C. (at least not later, though possibly, and according to many scholars, probably, far earlier),—the two divisions of Babylonia were separate monarchies. Thus, evidence whether any literature was written before or after B.C. 2000, may be found in the presence or absence of mention, or traces, of this division of the Babylonian kingdom. Mr. Smith considers, for example, that two works,—the great Chaldaean work on astrology, and a legend which he calls “The Exploits of Lubara,”—certainly belong to the period preceding B.C. 2000. In the former work, the sub-

ject of which specially connects it, as will presently be seen, with the tablet relating to the creation of the heavenly bodies, Akkad is always referred to as a separate state.

Now Mr. Smith finds that the story of the Creation and Fall belongs to the upper or Akkad division of the country. The Izdubar legends, containing the story of the Flood, and what Mr. Smith regards as probably the history of Nimrod, seem to belong to Sumir, the southern division of Babylonia. He considers the Izdubar legends to have been written at least as early as B.C. 2000. The story of the Creation "may not have been committed to writing so early;" but it also is of great antiquity. And these legends "were traditions before they were committed to writing, and were common, in some form, to all the country." Remembering Mr. Smith's expressed intention of setting all dates as late as possible, his endeavour to do this rather than to establish any system of chronology, we cannot misunderstand the real drift of his arguments, or the real significance of his conclusion that the period when the Genesis tablets were originally written extended from B.C. 2000 to B.C. 1550, or roughly synchronized with the period from Abraham to Moses, according to the ordinary chronology of our Bibles. "During this period it appears that traditions of the creation of the universe, and human history down to the time of Nimrod, existed parallel to, and in some points identical with, those given in the book of Genesis."

Thus viewing the matter, we recognize the interest of that passage in the Babylonian Genesis tablets which corresponds with the account in the book of Genesis respecting the creation of the heavenly bodies. We find in it the earliest existent record of the origin of astrological superstitions. It does not express merely the vague belief, which might be variously interpreted, that the sun and moon and stars were specially created (after light had been created, after the firmament had been formed separating the waters above from the waters below, and after the land had been separated from the water) to be for signs and for seasons for the in-

habitants of the world—that is, of our earth. It definitely states that those other suns, the stars, were set into constellation figures for man's benefit; the planets and the moon next formed for his use; and the sun set thereafter in the heavens as the chief among the celestial bodies.

It runs thus, so far as the fragments have yet been gathered together:—

#### FIFTH TABLET OF CREATION LEGEND.

1. It was delightful all that was fixed by the great gods.
2. Stars, their appearance [in figures] of animals he arranged,
3. To fix the year through the observation of their constellations,
4. Twelve months (or signs) of stars in three rows he arranged,
5. From the day when the year commences unto the close.
6. He marked the positions of the wandering stars (planets) to shine in their courses,
7. That they may not do injury, and may not trouble any one.
8. The positions of the gods Bel and Hea he fixed with him.
9. And he opened the great gates in the darkness shrouded,
10. The fastenings were strong on the left and right.
11. In its mass (*i.e.* the lower chaos) he made a boiling.
12. The god Uru (the moon) he caused to rise out, the night he overshadowed,
13. To fix it also for the light of the night until the shining of the day,
14. That the month might not be broken, and in its amount be regular.
15. At the beginning of the month, at the rising of the night,
16. His horns are breaking through to shine on the heaven.
17. On the seventh day to a circle he begins to swell,
18. And stretches towards the dawn further.
19. When the god Shamas (the sun) in the horizon of heaven, in the east,
20. . . . . formed beautifully and . . . . .
21. . . . . to the orbit Shamas was perfected
22. . . . . the dawn Shamas should change
23. . . . . going on its path
24. . . . . giving judgment
25. . . . . to tame
26. . . . . a second time
27. . . . .

Of this tablet Smith remarks that it is a typical specimen of the style of the series, and shows a marked stage in the

Creation, the appointment of the heavenly orbs running parallel to the biblical account of the fourth day of Creation. It is important to notice its significance in this respect. We can understand now the meaning underlying the words, "God said, Let there be lights in the firmament of the heavens, to divide the day from the night; and let them be for signs and for seasons, and for days and years." The order, indeed, in which the bodies are formed according to the biblical account is inverted. The greater light—the sun—is made first, to rule the day: then the lesser light—the moon—to rule the night. These are the heavenly bodies which in this description rule the day of 24 hours. The sun may be regarded also as ruling (according to the ancient view, as according to nature) the seasons and the year. The stars remain as set in the heaven for signs. "He made the stars also." "And God set them"—that is, the sun, moon, and stars—"in the firmament of the heaven to give light upon the earth, and to rule over the day and over the night," and so forth.

No one can doubt, I conceive, that the biblical account is superior to the other, both in a scientific and in a literary sense. It states much less as actually known, and what it does state accords better with the facts known in the writer's day. Then, the Babylonian narrative, though impressive in certain passages, is overloaded with detail. In both accounts we find the heavenly bodies set in the firmament by a special creative act, and specially designed for the benefit of man. And in passing I would observe that the discovery of these Babylonian inscriptions, however they may be interpreted, and whether they be regarded as somewhat earlier or somewhat later than the Bible narrative, appears to dispose finally of the fantastic interpretation assigned by Hugh Miller and others to the biblical cosmogony, as corresponding to a series of visions in which the varying aspects of the world were presented. It has long seemed to me an utterly untenable proposition that a narrative seemingly intended to describe definitely a certain series of

events should, after being for ages so interpreted, require now for its correct interpretation to be regarded as an account of a series of visions. If the explanation were reconcilable in any way with the words of Genesis, there yet seems something of profanity in imagining that men's minds had thus been played with by a narrative purporting to be of one sort yet in reality of quite a different character. But whatever possibility there may be (and it can be but the barest possibility) that the Genesis narrative admits of the vision interpretation, no one can reasonably attempt to extend that interpretation to the Babylonian account. So that either a narrative from which the Genesis account was presumably derived was certainly intended to describe a series of events, or else a narrative very nearly as early as the Genesis account, and presumably derived from it at a time when its true meaning must have been known, presents the sun, moon, and stars as objects expressly created and set in the sky after the earth had been formed, and for the special benefit of man as yet uncreated.

I am not concerned, however, either to dwell upon this point, or to insist on any of its consequences. Let us return to the consideration of the Babylonian narrative as it stands.

We find twelve constellations or signs of the zodiac are mentioned as set to fix the year. I am inclined to consider that the preceding words, "stars, their appearance in figures of animals he arranged," relate specially to the stars of the zodiac. The inventor of this astrogyony probably regarded the stars as originally scattered in an irregular manner over the heavens,—rather as chaotic material from which constellations might be formed, than as objects separately and expressly created. Then they were taken and formed into figures of animals, set in such a way as to fix the year through the observation of these constellations. It is hardly necessary, perhaps, to remind the reader that the word zodiac is derived from a Greek word signifying an animal, the original name of the zone being the zodiacal way, or the pathway of the animals. Our older navigators called it

the Bestiary.\* “Twelve months or signs in three rows.” Smith takes the three rows to mean (i.) the zodiacal signs, (ii.) the constellations north of the zodiac, and (iii.) the constellations south of the zodiac. But this does not agree with the words “twelve signs in three rows.” Possibly the reference is to three circles, two bounding the zodiac on the north and south respectively, the third central, the ecliptic, or track of the sun; or the two tropics and the equator may have been signified. Instead of “twelve signs in three rows,” we should, probably, read “twelve signs along a triple band.” The description was written long after astronomical temples were first erected, and as the designer of a zodiacal dome like that (far more recently) erected at Denderah would set the twelve zodiacal signs along a band formed by three parallel circles, marking its central line and its northern and southern limits, so we can understand the writer of the tablet presenting the celestial architect as working in the same lines, on a grander scale; setting the twelve zodiacal signs on the corresponding triple band in the heavens themselves.

The next point to be noticed in the Babylonian astrology

\* The following passage from Admiral Smyth's Bedford Catalogue is worth noticing in this connection:—“We find that both the Chinese and the Japanese had a zodiac consisting of animals, as *zodiacs* needs must, among which they placed a tiger, a peacock, a cat, an alligator, a duck, an ape, a hog, a rat, and what not. Animals also formed the *Via Solis* of the Kirghis, the Mongols, the Persians, the Mantshus, and the ancient Turks; and the Spanish monks in the army of Cortes found that the Mexicans had a zodiac with strange creatures in the departments. Such a striking similitude is assuredly indicative of a common origin, since the coincidences are too exact in most instances to be the effect of chance; but where this origin is to be fixed has been the subject of interminable discussions, and learning, ignorance, sagacity, and prejudice have long been in battle array against each other. Diodorus Siculus considers it to be Babylonian, but Bishop Warburton, somewhat dogmatically tells us, ‘Brute worship gave rise to the Egyptian asterisms prior to the time of Moses.’” There is now, of course, very little reason for questioning that Egyptian astronomy was borrowed from Babylon.

is the reference to "wandering stars." Mr. Smith remarks that the word *nibir*, thus translated, "is not the usual word for planet, and there is a star called *Nibir* near the place where the sun crossed the boundary between the old and new years, and this star was one of twelve supposed to be favourable to Babylonia." "It is evident," he proceeds, "from the opening of the inscription on the first tablet of the Chaldaean astrology and astronomy, that the functions of the stars were, according to the Babylonians, to act not only as regulators of the seasons and the year, but also to be used as signs, as in Genesis i. 14; for in those ages it was generally believed that the heavenly bodies gave, by their appearance and positions, signs of events which were coming on the earth." The two verses relating to Nibir seem to correspond to no other celestial bodies but planets (unless, perhaps, to comets). If we regard Nibir as signifying any fixed star, we can find no significance in the marking of the course of the star Nibir, that it may do no injury and may not trouble any one. Moreover, as the fixed stars, the sun, and the moon, are separately described, it seems unlikely that the planets would be left unnoticed. In the biblical narrative the reference to the celestial bodies is so short that we can understand the planets being included in the words, "He made the stars also." But in an account so full of detail as that presented in the Babylonian tablet, the omission of the planets would be very remarkable. It is also worthy of notice that in Polyhistor's Babylonian traditions, recorded by Berosus, we read that "Belus formed the stars, the sun, the moon, and the five planets."

In the tablet narrative the creator of the heavenly bodies is supposed to be Anu, god of the heavens. This is inferred by Mr. Smith from the fact "that the God who created the stars, fixed places or habitations for Bel and Hea with himself in the heavens." For according to the Babylonian theogony, the three gods Anu, Bel, and Hea share between them the divisions of the face of the sky.

The account of the creation of the moon is perhaps the

most interesting part of the narrative. We see that, according to the Babylonian philosophy, the earth is regarded as formed from the waters and resting after its creation above a vast abyss of chaotic water. We find traces of this old hypothesis in several biblical passages, as, for instance, in the words of the Third Commandment, "the heaven above, the earth beneath, and the waters under the earth;" and again in Proverbs xxx. 4, "Who hath bound the waters in a garment? who hath established all the ends of the earth?" "The great gates in the darkness shrouded, the fastenings strong on the left and right," in the Babylonian account, refer to the enclosure of the great infernal lake, so that the waters under the earth might not overwhelm the world. It is from out the dark ocean beneath the earth that the god Anu calls the moon into being. He opens the mighty gates shrouded in the nether darkness, and creates a vast whirlpool in the gloomy ocean; then "at his bidding, from the turmcil arose the moon like a giant bubble, and passing through the open gates mounted on its destined way across the vaults of heaven." It is strange to reflect that in quite recent times, at least 4000 years after the Babylonian tablet was written, and who shall tell how many years after the tradition was first invented?—a theory of the moon's origin not unlike the Babylonian hypothesis has been advanced, despite overwhelming dynamical objections; and a modern paradoxist has even pointed to the spot beneath the ocean where a sudden increase of depth indicates that matter was suddenly extruded long ago, and driven forcibly away from the earth to the orbit along which that expelled mass—our moon—is now travelling.

It would have been interesting to have known how the Babylonian tablet described the creation of Shamas, the sun; though, so far as can be judged from the fragments above quoted, there was not the same fulness of detail in this part of the description as in that relating to the moon. Mr. Smith infers that the Babylonians considered the moon the more important body, unlike the writer or compiler of

the book of Genesis, who describes the sun as the greater light. It does not seem to follow very clearly, however, from the tablet record, that the sun was considered inferior to the moon in importance, and certainly we cannot imagine that the Babylonians considered the moon a greater light. The creation of the stars precedes that of the moon, though manifestly the moon was judged to be more important than the stars. Not improbably, therefore, the sun, though following the moon in order of creation, was regarded as the more important orb of the two. In fact, in the Babylonian as in the (so-called) Mosaic legend of Creation, the more important members of a series of created bodies are, in some cases, created last—man last of all orders of animated beings, for instance.

If we turn now from the consideration of the Babylonian tradition of the creation of the heavenly bodies to note how the biblical account differs from it, not only or chiefly in details, but in general character, we seem to recognize in the latter a determination to detach from the celestial orbs the individuality, so to speak, which the older tradition had given to them. The account in Genesis is not only simpler, and, in a literary sense, more effective, but it is in another sense purified. The celestial bodies do not appear in it as celestial beings. The Babylonian legend is followed only so far as it can be followed consistently with the avoidance of all that might tempt to the worship of the sun, moon, and stars. The writer of the book of Genesis, whether Moses or not, seems certainly to have shared the views of Moses as to the Sabæanism of the nation from which the children of Abraham had separated. Moses warned the Israelite,—“Take good heed unto thyself, lest thou lift up thine eyes unto heaven; and when thou seest the sun, and the moon, and the stars, even all the host of heaven, shouldest be driven to worship them, and serve them, which the Lord thy God hath divided unto all nations under the whole heaven.” So the writer of Genesis is careful to remove from the tradition which he follows all that might suggest the individual

power and influence of the heavenly bodies. The stars are to be for signs, but we read nothing of the power of the wandering stars "to do injury or trouble any one." (That is, not in the book of Genesis. In the song of Deborah we find, though perhaps only in a poetic fashion, the old influences assigned to the planets, when the singer says that the "stars in their courses fought against Sisera." Deborah, however, was a woman, and women have always been loth and late to give up ancient superstitions.) Again, the sun and the moon in Genesis are the greater and the lesser lights, not, as in the Babylonian narrative, the god Shamas and the god Uru.

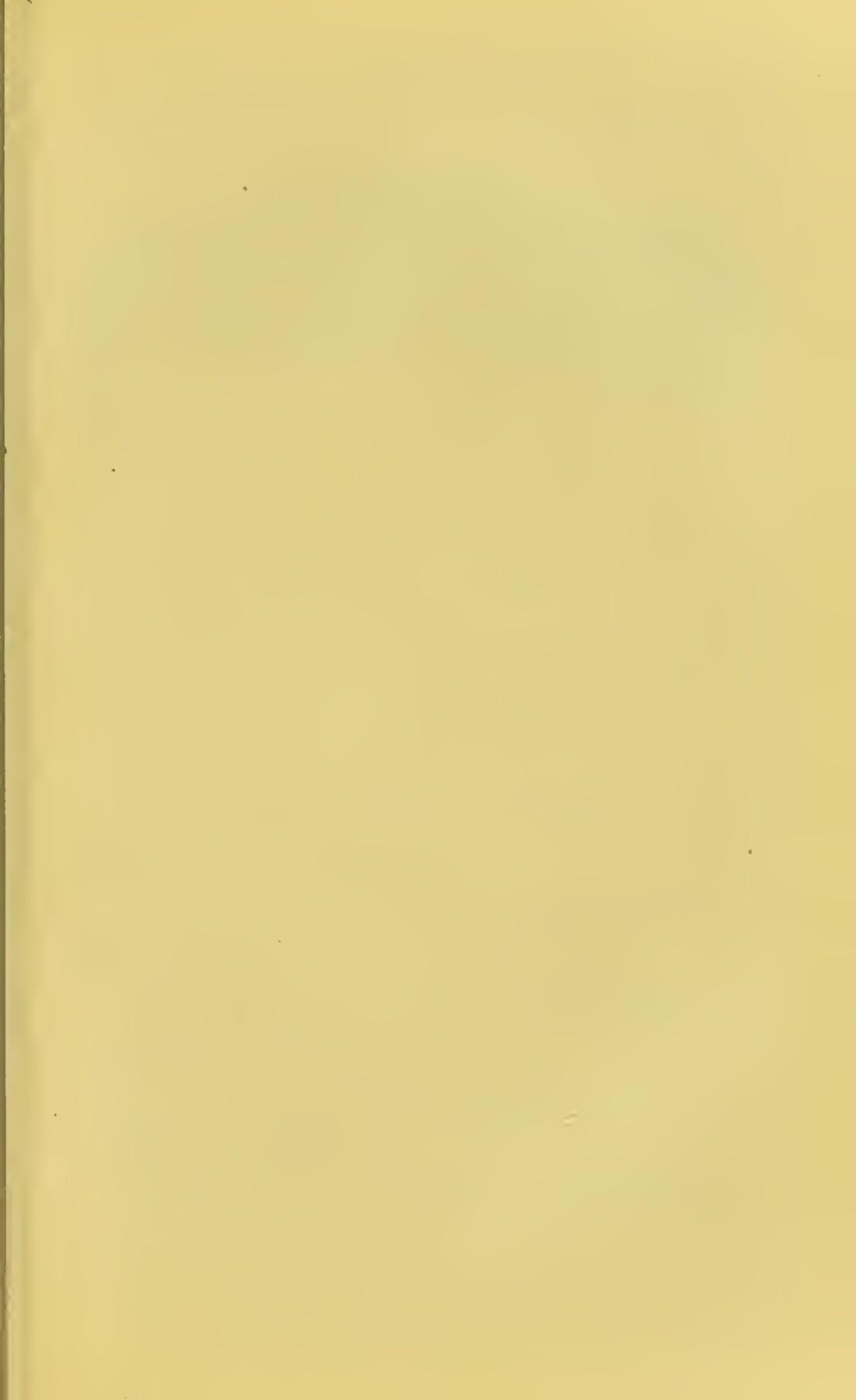
We may find a parallel to this treatment of the Babylonian myth in the treatment by Moses of the observance of the Sabbath, a day of rest which the Babylonian tablets show to have had, as for other reasons had been before suspected, an astrological significance. The Jewish lawgiver does not do away with the observance; in fact, he was probably powerless to do away with it. At any rate, he suffers the observance to remain, precisely as the writer of the book of Genesis retains the Babylonian tradition of the creation of the celestial bodies. But he is careful to expurgate the Chaldæan observance, just as the writer of Genesis is careful to expurgate the Babylonian tradition. The week as a period is no longer associated with astrological superstitions, nor the Sabbath rest enjoined as a fetish. Both ideas are directly associated with the monotheistic principle which primarily led to the separation of the family of Abraham from the rest of the Chaldæan race. In Babylonia, the method of associating the names of the sun, moon, and stars with the days, doubtless had its origin. Saturn was the Sabbath star, as it is still called (*Sabbatai*) in the Talmud. But, as Professor Tischendorf told Humboldt, in answer to a question specially addressed to him on the subject, "there is an entire absence in both the Old and New Testaments, of any traces of names of week-days taken from the planets." The lunar festivals, again, though unquestionably Sabaistic

in their origin, were apparently too thoroughly established to be discarded by Moses ; nay, he was even obliged to permit the continuance of many observances which suspiciously resembled the old offerings of sacrifice to the moon as a deity. He had also to continue the sacrifice of the pass-over, the origin of which was unmistakably astronomical,—corresponding in time to the sun's passage across the equator, or rather to the first lunar month following and including that event. But he carefully dissociates both the lunar and the lunisolar sacrifices from their primary Sabaistic significance. In fact, the history of early Hebrew legislation, so far as it related to religion, is the history of a struggle on the part of the lawgivers and the leaders of opinion against the tendency of the people to revert to the idolatrous worship of their ancestors and of races closely akin to them—especially against the tendency to the worship of the sun and moon and all the host of heaven.

In the very fact, however, that this contest was maintained, while yet the Hebrew cosmogony, and in particular the Hebrew astrogyony, contains indubitable evidence of its origin in the poetical myths of older Babylonia, we find one of the strongest proofs of the influence which the literature of Babylon, when at the fulness of its development, exerted upon surrounding nations. This influence is not more clearly shown even by the fact that nearly 2000 years after the decay of Babylonian literature, science, and art, a nation like the Assyrians, engaged in establishing empire rather than in literary and scientific pursuits, should have been at the pains to obtain copies of many thousands of the tablet records which formed the libraries of older Babylonia. In both circumstances we find good reason for hoping that careful search among Assyrian and Babylonian ruins may not only be rewarded by the discovery of many other portions of the later Assyrian library (which was also in some sense a museum), but that other and earlier copies of the original Babylonian records may be obtained. For it seems unlikely that works so valuable as to be thought worth recopying after

1500 or 2000 years, in Assyria, had not been more than once copied during the interval in Babylonia. "Search in Babylonia," says Mr. Smith, "would no doubt yield earlier copies of all these works, but that search has not yet been instituted, and for the present we have to be contented with our Assyrian copies. Looking, however, at the world-wide interest of the subjects, and at the important evidence which perfect copies of these works would undoubtedly give, there can be no doubt," Mr. Smith adds, "that the subject of further search and discovery will not slumber, and that all as yet known will one day be superseded by newer texts and fuller and more perfect light."







March, 1881.



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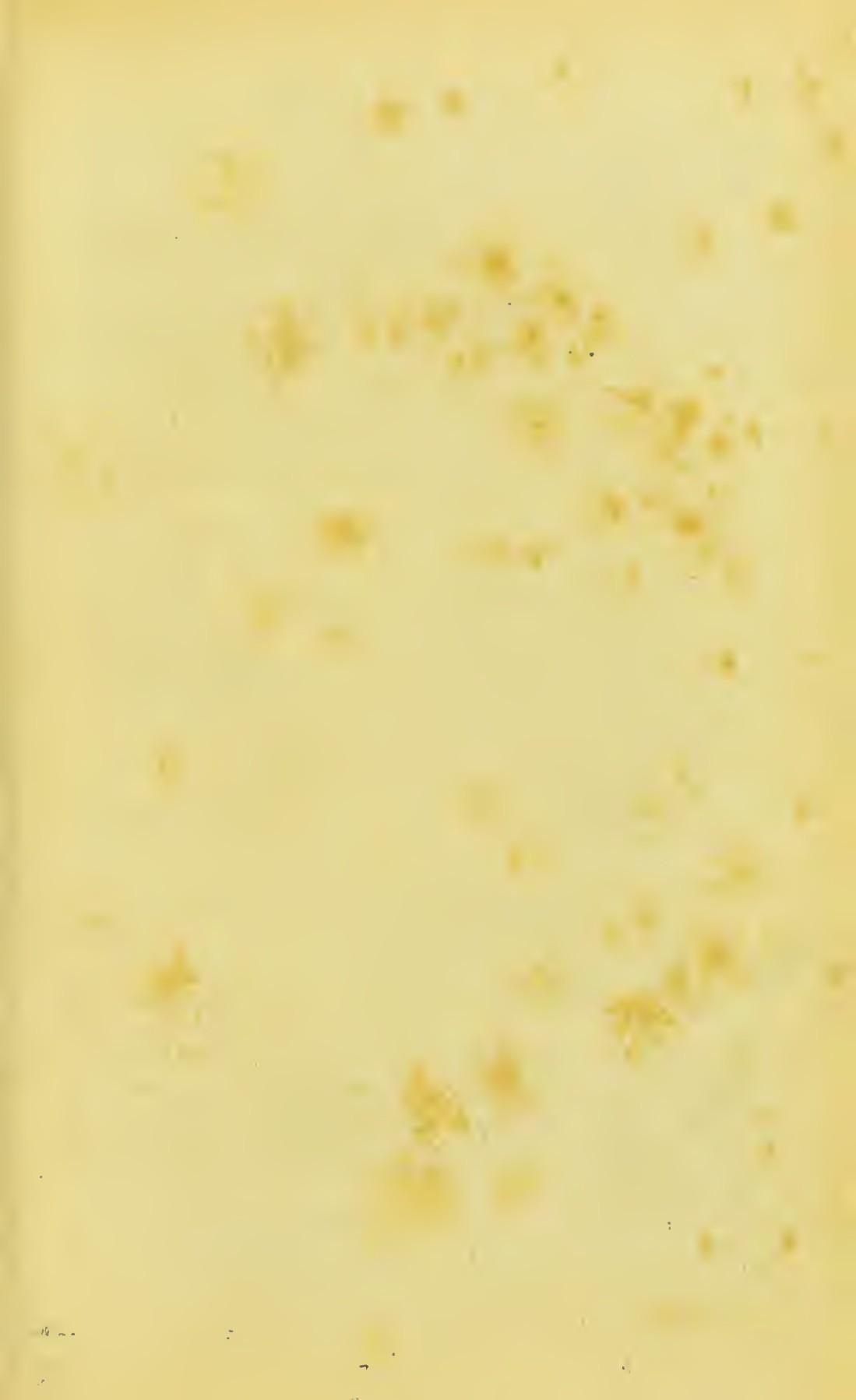
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